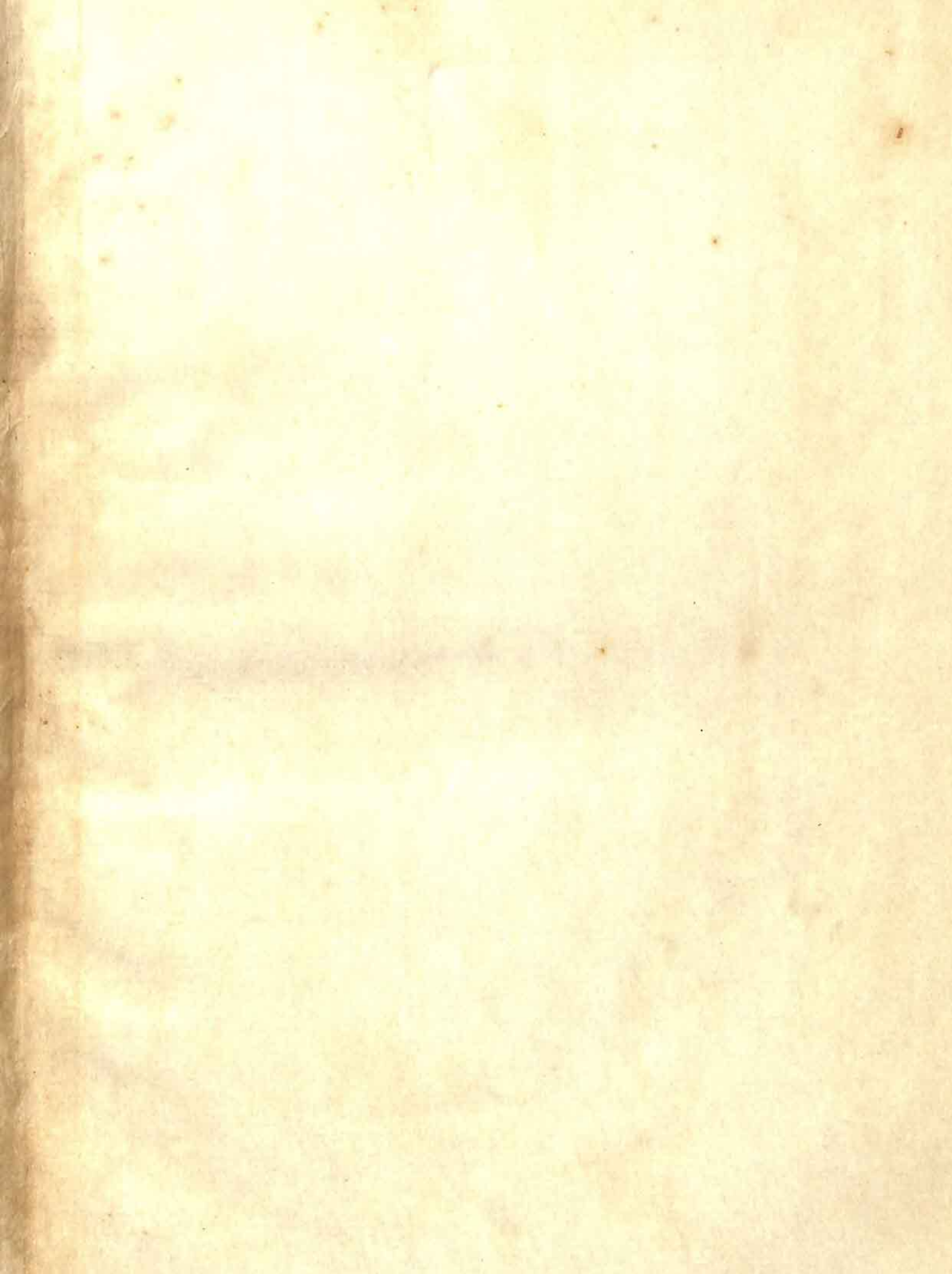


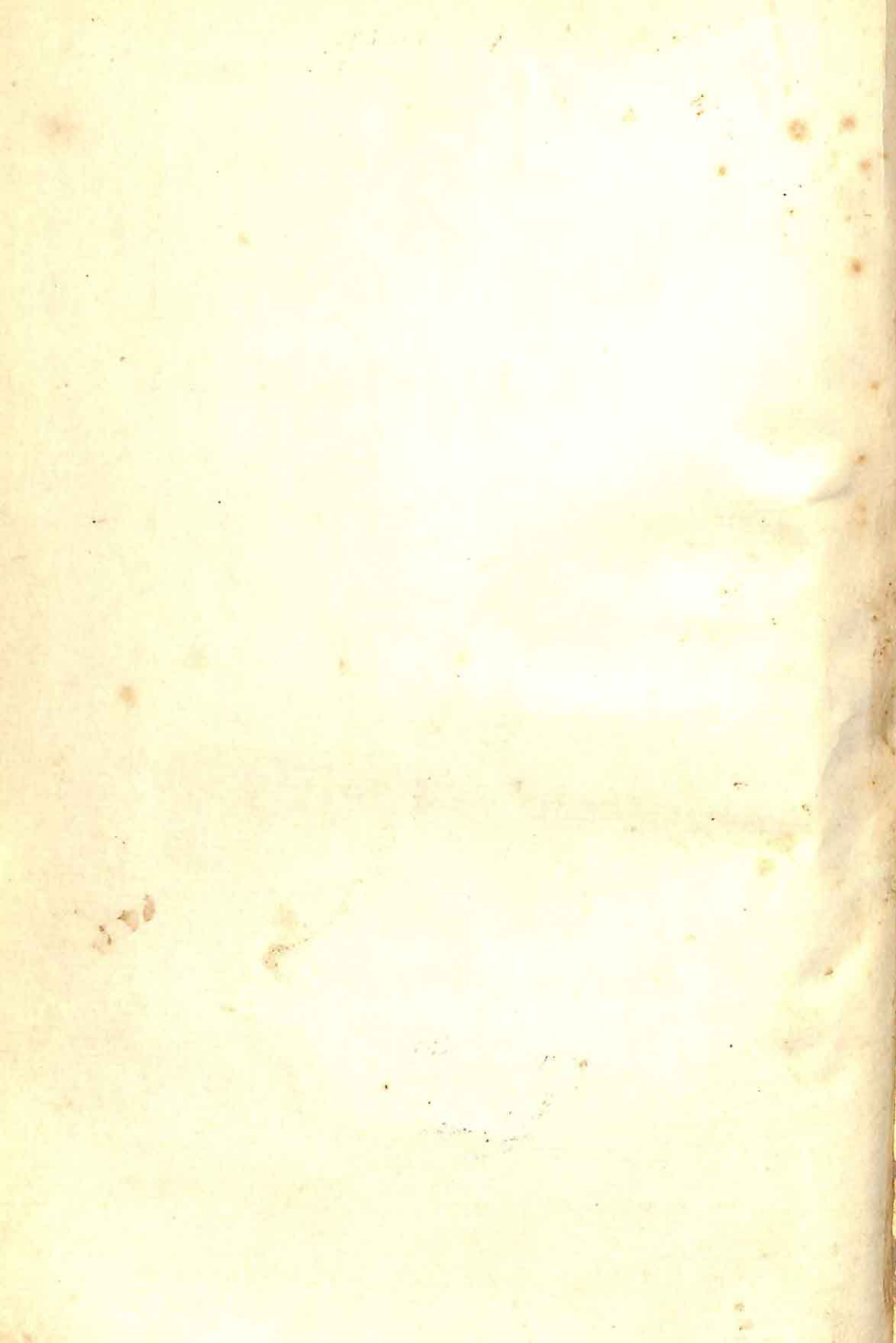
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CONTENTS

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29 APR 1961

CONTENTS

A. M. HALLIDAY and R. MINGAY. Retroactive Raising of a Sensory Threshold by a Contralateral Stimulus - - - -	1
C. I. HOWARTH and M. TREISMAN. Lowering of an Auditory Threshold by a Near Threshold Warning Signal - - - -	12
I. P. HOWARD. An Investigation of a Satiation Process in the Reversible Perspective of Revolving Skeletal Shapes - - - -	19
C. G. GROSS and L. WEISKRANTZ. The Effect of Two "Tranquillizers" on Auditory Discrimination and Delayed Response Performance of Monkeys - - - - -	34
P. C. DODWELL. Visual Orientation Preferences in the Rat -	40

Shorter Articles and Notes

N. O'CONNOR and B. HERMELIN. Like and Cross Modality Recognition in Subnormal Children - - - - -	48
E. A. LAWSON. A Note on the Influence of Different Orders of Approximation to the English Language upon Eye-Voice Span -	53
Experimental Psychology Monographs - - - - -	56
Proceedings of the Experimental Psychology Society, 1960 - -	56
Book Reviews - - - - -	58
Publications Received - - - - -	63
Correspondence - - - - -	64

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ALBERT WILLIAM WOLTERS, 1883-1961.

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CONTENTS

Papers on Figural After-Effects

R. H. DAY and J. A. LOGAN. A Further Investigation of Apparent Size and Retinal Size as Determinants of the Figural After-Effect	- 193
ANNE W. STORY. Has Apparent Size Been Tested as a Factor in Figural After-Effects?	- 204
R. F. TERWILLIGER. Retinal Size as a Determiner of the Direction of Size Distortion in Figural After-Effects	- 209
J. WILSON. Is the Figural After-Effect Accompanied by Changes in Discrimination?	- 218
N. S. SUTHERLAND. Figural After-Effects and Apparent Size	- 222

Shorter Articles and Notes

N. F. DIXON and M. HAIDER. Changes in the Visual Threshold as a Function of Subception	- 229
C. I. HOWARTH and K. ELLIS. The Relative Intelligibility Threshold for One's Own Name Compared with Other Names	- 236
A. ELITHORN. Central Intermittency: Some Further Observations	- 240
Book Reviews	- 248
Index of Authors	- 255

The Quarterly Journal of Experimental Psychology is published for the Experimental Psychology Society by W. Heffer & Sons Ltd., Petty Cury, Cambridge, England, to whom all business communications should be addressed.

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- M. D. VERNON. The relation of perception to personality factors.
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CONTENTS

H. P. ZEIGLER and L. B. WYCKOFF, Jr. Observing Responses and Discrimination Learning - - - - -	129
R. A. CHASE, S. HARVEY, S. STANDFAST, I. RAPIN and S. SUTTON. Studies on Sensory Feedback. I: Effect of Delayed Auditory Feedback on Speech and Keytapping - - - - -	141
R. A. CHASE, I. RAPIN, L. GILDEN, S. SUTTON and G. GUILFOYLE. Studies on Sensory Feedback. II: Sensory Feedback Influences on Keytapping Motor Tasks - - - - -	153
<i>Shorter Articles and Notes</i>	
M. J. PICKERSGILL. On Knowing With Which Eye One is Seeing	168
H. SAMPSON and P. SPONG. Handedness, Eye-Dominance and Immediate Memory - - - - -	173
<i>Apparatus</i>	
R. M. PRITCHARD. A Collimator Stabilizing System for the Retinal Image - - - - -	181
Book Reviews - - - - -	184

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CONTENTS

W. SLUCKIN and E. A. SALZEN. Imprinting and Perceptual Learning - - - - -	65
R. DAVIS, N. MORAY and ANNE TREISMAN. Imitative Responses and the Rate of Gain of Information - - - - -	78
P. BERTELSON. Sequential Redundancy and Speed in a Serial Two-choice Responding Task - - - - -	90
<i>Shorter Articles and Notes</i>	
D. E. BROADBENT and MARGARET GREGORY. On the Recall of Stimuli Presented Alternately to Two Sense-Organs - - - - -	103
N. MORAY. A Note on Broadbent and Gregory's Paper - - - - -	110
ROCHELLE M. ESKIN and M. E. BITTERMAN. Partial Reinforcement in the Turtle - - - - -	112
N. S. SUTHERLAND. Visual Discrimination of Horizontal and Vertical Rectangles by Rats on a New Discrimination Training Apparatus	117
G. F. REED and A. C. SMITH. Laterality and Directional Preferences in a Simple Perceptual-Motor Task - - - - -	122
Book Reviews - - - - -	125

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Part 1

RETROACTIVE RAISING OF A SENSORY THRESHOLD BY A CONTRALATERAL STIMULUS

BY

A. M. HALLIDAY and ROSEMARY MINGAY

*From the M.R.C. Neurological Research Unit, The National Hospital,
Queen Square, W.C.1*

In an experiment designed to extend Howarth and Treisman's recent observations on the lowering of the visual and auditory threshold produced by a warning stimulus, it has been found that the threshold to electrical stimulation of one forearm is *raised* by a similar stimulus on the other forearm. As in the earlier experiments the "warning" was effective if it came 100 millisecon. before or after the threshold stimulus. The effect increased with increasing warning strength over the range of five near-threshold strengths used.

These results appear to be inconsistent with the explanation previously proposed to account for the retroactive lowering of threshold. This suggested that the warning was acting simply as a marker, reducing the subject's uncertainty as to when the stimulus was coming. The fact that the rise in threshold was also induced retroactively shows that some interaction must be occurring in the central processes concerned in perception, and that it cannot be explained in terms of attenuation of the afferent volley.

INTRODUCTION

Recent work has shown that a sensory volley may be modified in size while passing through the synapses on its way to the cerebral cortex, and it has been suggested that the centrifugal control of these neuronal relays by the higher centres may contribute an important part of the mechanism of attention (see, for example, Adrian, 1958; Dawson, 1958). This centrifugal control may either diminish or increase the size of the afferent volley. But the size of the afferent volley cannot be the only factor determining whether a given sensation reaches consciousness, for the evoked cortical potential may not be reduced in size in deep anaesthesia (Brazier, 1954) or when a patient with an "inattention defect" associated with damage to the parietal lobe can no longer perceive a stimulus presented on the affected side owing to simultaneous contralateral stimulation (Giblin, 1960). A similar finding of apparently normal evoked potentials on the affected side has been reported in cases of hysterical hemianaesthesia (Alajouanine, Scherrer, Barbizet, Calvet and Verley, 1958).

The centrifugal control of sensory input on its way to the cortex also fails to account for the fact that the perception of a stimulus can be affected by a second stimulus coming shortly afterwards (Crawford, 1947; Treisman and Howarth, 1959), as in this case the afferent volley from the first stimulus would be expected to reach the cortex before the second volley could catch up with it and interact.

Treisman and Howarth (1959), who found that a visual or auditory stimulus significantly lowered the threshold of a stimulus given in the other modality even if

it came 100 to 500 millisecon. afterwards, have suggested a completely different hypothesis to account for this particular effect. It is supposed that the second (or "warning") signal acts as a kind of marker reducing the subject's uncertainty as to when the critical stimulus is coming. Since the time estimation error obeys Weber's Law (Woodrow, 1930), the length of time over which the subject will expect the critical stimulus (or, alternatively, the amount it deviates in successive trials from the time that he expects it) will increase with increasing warning interval and will do so in the same way whether the warning precedes or follows the critical stimulus; it will be at its lowest with a simultaneous warning. The shorter the interval to be monitored the lower the threshold criterion can afford to be set without increasing the probability of false positive signals due to noise in the channel. It is assumed in this explanation that the sensory threshold is closely related to the noise level in the system and that stimulus detection is a process of discrimination between signal and noise. The predictions of this theory seem to fit very well the results of Treisman and Howarth which show a maximum lowering of threshold when the two stimuli are simultaneous and an effect diminishing with increasing interstimulus interval in a similar way whether the "warning" signal precedes or follows the critical stimulus.

The hypothesis does not seem able to cover the results of Crawford (1947), who showed that the threshold for a visual stimulus was raised by a "conditioning" stimulus fed into the same eye up to 100 millisecon. later. But this finding of Crawford's was only incidental to an investigation of the course of visual adaption following the conditioning stimulus, and no other observations have been found in the literature bearing on the possible existence of retroactive raising of the sensory threshold by a closely following stimulus. A rather similar and perhaps allied phenomenon has, however, been observed in patients with an "inattention defect" due to a lesion of the parietal lobe, where a stimulus on the affected side can be rendered imperceptible by a contralateral stimulus occurring more than 100 millisecon. later (Unpublished observations).

In the present experiment we have investigated the effect of an electrical stimulus (the "warning" stimulus) to the back of one forearm on the threshold of a similar stimulus (the critical stimulus) to the opposite side. The "warning" stimulus differed from the critical stimulus in that it was always perceptible, whereas the critical stimulus was given in strengths both above and below the initial threshold and was omitted altogether on a percentage of trials. The subject was required to report on the presence or absence of the critical stimulus. As in Howarth and Treisman's experiment (Howarth and Treisman, 1958; Treisman and Howarth, 1959) the interstimulus interval has been varied only from run to run, the subject being told before each run what interval to expect.

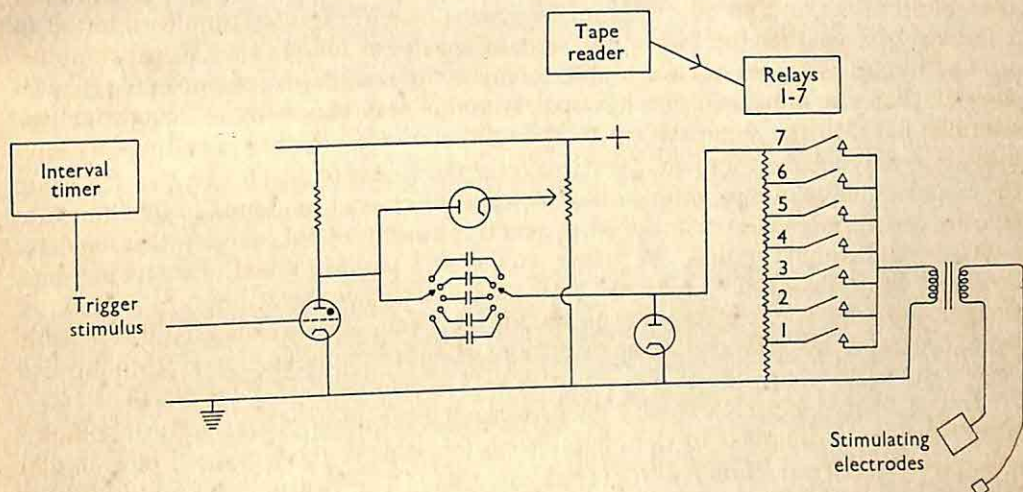
Although the "warning" stimulus was always given at suprathreshold strengths, its strength was varied in order to test the following prediction. If the warning is acting simply as a time marker, it would seem to make little difference how strong it is, provided that it is perceived. But if the change in threshold for the critical stimulus is induced by some central process, either excitatory or inhibitory, then the strength of the "warning" stimulus (in so far as this determines the strength of the central events which it sets up) may determine the magnitude of the change in the critical threshold. The experiment was planned in expectation of a lowering of threshold by the warning. In the event a raising of threshold was observed. The effect varied with varying warning strength and was retroactive. This does not provide a direct test of the validity of the marker theory in the case of lowering of the threshold. It does, however, make it less likely. This question will be considered more fully in the discussion.

METHOD AND PROCEDURE

The electrical stimuli were condenser discharges of brief duration (time constant 10 or 20 μ sec.) derived from a two-channel thyatron stimulator. One channel is shown diagrammatically in Figure 1. The skin was prepared and two rectangular silver electrodes were strapped on the back of each forearm, a smaller one (1.7×2 cm.), placed distally, serving as cathode, while a larger one (3.75×6 cm.) more proximally, served as anode. The two sides were supplied by separate channels through isolating transformers, so that there was no common earth return path and the stimuli were completely independent.

The stimulator channel supplying the "critical" stimulus fed a stepped output attenuator (Figure 1) with seven outputs increasing in 1 dB. steps; any of these seven output strengths could be chosen by a series of seven relays operated from punched tape. A similar, independently triggered, channel with only five output strengths, each also 1 dB. apart, supplied the "warning" stimulus. Both attenuators had a maximum output impedance of 250 ohms.

FIGURE 1



Schematic diagram of one channel of the thyatron stimulator. The output strength is selected by means of relays switching the output attenuator (1 dB. steps), but the initial setting of threshold is made by means of the potentiometer controlling the return voltage of the thyatron anode.

After the skin resistance had been measured, an initial threshold determination was made for each side separately by the ascending and descending method of limits; during this determination the stimulus was regularly repeated every 1 or 2 sec. The initial threshold was taken as the stimulus strength at which the subject could just perceive all the regularly repeated stimuli (i.e. the 100 per cent. threshold). It will be noted from Figure 1 that the output voltage of the stimulator could be varied by the experimenter without affecting the output attenuator ratios, by means of the potentiometer altering the "return" voltage to which the stimulating condenser was recharged between stimuli. For the critical stimulus, the initial threshold was determined using the fourth of the seven attenuator outputs, so that these outputs (subsequently used in the experiment itself) were now arranged at -3 dB., -2 dB., -1 dB., 0, +1 dB., +2 dB. and +3 dB. about the initial threshold. The threshold for the "warning" stimulus was determined for the strongest of the five attenuator outputs; this output voltage was then doubled (i.e. raised by 6 dB.) for the experiment proper, so that the warning strengths used were +2 dB., +3 dB., +4 dB., +5 dB. and +6 dB. above the initial threshold. Both thresholds were determined again at the end of the experiment and the skin resistance was also measured. There appeared to be no association of any kind between such changes as were noted in threshold and resistance, and in most cases these changes were negligible.

Ten subjects, five men and five women, did the main experiment on two consecutive days. After the initial threshold determination they were given five runs of 100 situations each. In each run the five strengths of "warning" stimulus occurred 20 times in all, twice with each of the seven strengths of "critical" stimulus and six times with no critical stimulus. The order of presentation of these situations within the run was randomized and programmed from the punched tape. Before each situation was presented, the experimenter said "Ready," and the stimuli were given after a foreperiod varied randomly by the experimenter between approximately 2 to 7 sec.

In each of the five daily runs the interval between "warning" and "critical" stimuli was kept constant, but five different intervals were used for the five different runs, viz. the "warning" coming 500 millisecc. before the critical stimulus (" +500"), 100 millisecc. before (" +100"), simultaneously ("O"), 100 millisecc. after (" -100") and 500 millisecc. after (" -500"). The subject was told before each run which stimulus would be first and what the interval would be. The order of presentation of the intervals (runs) was the same for any one subject on both his days, but was varied for each of the 10 subjects in what seemed a balanced way, each interval occurring in each position in the series twice. The whole daily session, which included a few minutes rest between runs, took approximately 2 to 2½ hr.

The sides on which the two stimuli were given was reversed on the second day. Half the subjects started off with the critical stimulus on the right forearm, half with it on the left.

Some weeks later, the same 10 subjects also did a control experiment in which they came on only 1 day and did three runs of 100 situations each. In two of these runs, the simultaneous and " +500" intervals were repeated, while in the third the critical stimulus was given without a warning stimulus after the pre-warning of "Ready" and with the variable foreperiod as before. The same system of randomization was used as for the rest of the experiment, but all five warning "strengths" were now "blanks."

RESULTS

The subject was required to say "Yes," "No" or "Doubtful" to indicate whether he had felt the critical stimulus. In calculating the results the total number correct has been used (rather than the number perceived) and half the doubtfuls have been added to the number correct. This assumes that a doubtful response was equally likely to be right or wrong, but the results show that there was seldom much doubtfulness about absent stimuli. The method therefore probably slightly underestimates the number correct. The false positive rate was less than 0.5 per cent. and the percentage of "doubtful" verdicts in response to blanks less than 2 per cent. (Table I). "Doubtfuls" made up 9 per cent. of the responses to "critical" stimuli in the main experiment, and 7.1 per cent. in the control experiment (Table II). The very few errors and the lack of uncertainty about blanks show that the results obtained cannot be due to guessing.

TABLE I
TOTAL NUMBER OF FALSE POSITIVE AND DOUBTFUL
RESPONSES WHEN NO CRITICAL STIMULUS WAS GIVEN

Interval	Main experiment			Control experiment		
	False positives	Doubtfuls	N	False positives	Doubtfuls	N
+500	0	4	600	3	1	300
+100	1	8	600	—	—	—
0	3	14	600	1	2	300
-100	1	16	600	—	—	—
-500	4	6	600	—	—	—
No warning	—	—	—	0	2	300
Total	9 (0.3%)	48 (1.6%)	3000	4 (0.4%)	5 (0.6%)	900

TABLE II
PERCENTAGE OF DOUBTFUL RESPONSES TO CRITICAL STIMULI

<i>Interval</i>	<i>Main experiment</i>	<i>N</i>	<i>Control experiment</i>	<i>N</i>
	(percentages)		(percentages)	
+500	9.4	1400	5.8	700
+100	10.4	1400	—	—
0	9.3	1400	8.1	700
-100	9.5	1400	—	—
-500	6.3	1400	—	—
No warning	—	—	7.4	700
Total	9.00	7000	7.14	2100

An analysis of variance was first carried out on the total number of correct responses in the main experiment. This is shown in Table III (unbracketed figures). For the pooled data of all five runs, interval was significant at the 0.1 per cent. level, as was the side of the critical stimulus (laterality). Warning strength only reached the 5 per cent. level of significance on this data, but a similar analysis carried out on those runs with the three shorter intervals, where the warning is expected to be most effective, brought it to the 1 per cent. level. (Figures in brackets, Table III.)

TABLE III
ANALYSIS OF VARIANCE

<i>Source of variation</i>	<i>d.f.</i>	<i>F ratio</i>	<i>p less than</i>
Interval	4 (2)	13.93 (4.80)	0.001 (0.01)
Warning strength	4 (4)	2.45 (4.51)	0.05 (0.01)
Laterality	1 (1)	28.28 (15.13)	0.001 (0.001)
Interval $\times$ warning strength	16 (8)	0.79 (0.42)	† (†)
Interval $\times$ laterality	4 (2)	1.99 (2.00)	† (†)
Warning strength $\times$ laterality	4 (4)	0.29 (0.38)	† (†)
Interval $\times$ warning strength $\times$ laterality	16 (8)	1.37 (1.22)	† (†)
Residual	450 (270)	—	—

Figures in brackets give results for interstimulus intervals of +100, 0, -100 millisecc. only.

† *p* in these cases exceeded 0.1.

The significance of the laterality effect was unexpected and its further analysis is complicated by several uncontrolled factors. It was found after the experiment that two of the five subjects who started with the critical stimulus on the left side happened to be left-handed, while all five of those who started with it on the right were right-handed. It is possible that the significant laterality effect may represent an effect of cerebral dominance on perception or an interaction between dominance and practice effects, but there were marked individual differences and our results do not warrant any definite conclusions.

The effect of interstimulus interval on threshold

By plotting the mean percentage of correct responses for each of the seven "critical" stimulus strengths a threshold curve is obtained. This has been done in Figure 2 (a), separately for each of the five interstimulus intervals. These threshold curves do not cover the whole range from 0 to 100 per cent. felt, as an appreciable number of the weakest stimuli are being perceived. (Since the false positive rate is virtually negligible, these cannot be guesses.) The region of threshold uncertainty seems likely to cover about 6 to 7 dB.

FIGURE 2 a and b

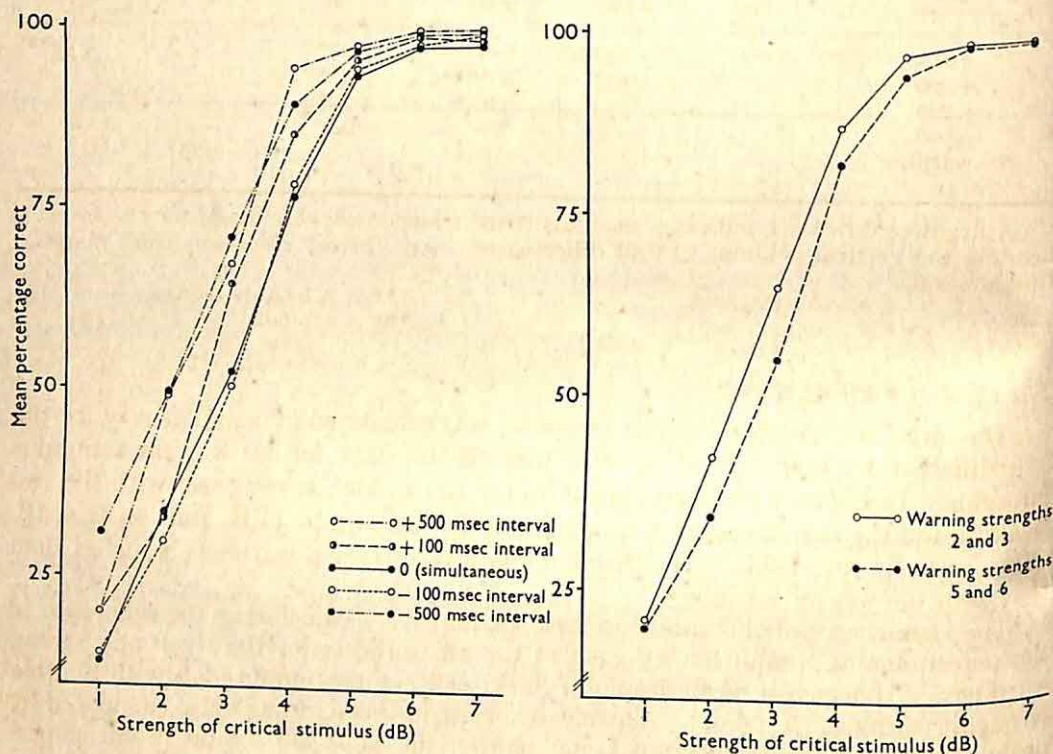


Figure 2 (a). Mean threshold curves for each of the five interstimulus intervals. "+" indicates that the "warning" stimulus precedes the critical, and "-" that it follows. Figure 2 (b) shows similar curves for the two weakest and the two strongest strengths of warning stimuli (+2, +3 and +5, +6 dB. above initial warning threshold respectively).

The curves for those runs with the three shorter warning intervals (" +100," "0," "-100") are consistently to the right of those for the two longer intervals (" +500," "-500"), indicating that the threshold is higher in the former case. The differences between the mean percentage correct with the different warning intervals are shown in Table IV, and are highly significant between the short and long intervals. Since more correct responses are made at the longer warning intervals, the interval effect is the opposite to that observed by Howarth and Treisman, who found more stimuli were perceived with the shorter warning intervals.

The results of the control experiment, shown in Table IV, confirm that in our situation a significantly greater number of stimuli were perceived at "+500" than at

"O." The "No warning" run differed significantly from the run with a simultaneous warning ($p < 0.05$), but not from the run with a warning preceding the critical stimulus by 500 millisecc. This implies that the threshold is being raised by the warning when it occurs at the shorter intervals and simultaneity.

TABLE IV
DIFFERENCES BETWEEN THE MEAN PERCENTAGE OF CORRECT RESPONSES
WITH DIFFERENT INTERSTIMULUS INTERVALS

Interval in millisec.	Main experiment				Control experiment	
	-100	+100	+500	-500	0	+500
0	-0.1	3.65**	6.8***	7.4***		5.55**
-100		3.55*	6.7***	7.3***		
+100			3.15*	3.75**		
+500				0.6		
No warning					-3.75†	1.8

A negative difference indicates that less correct responses were made for the interval heading the vertical column. In all other cases, more correct responses were made at this interval.

† $p < 0.05 > 0.02$.

* $p < 0.02 > 0.01$.

** $p < 0.01 > 0.001$.

*** $p < 0.001$.

The effect of warning strength

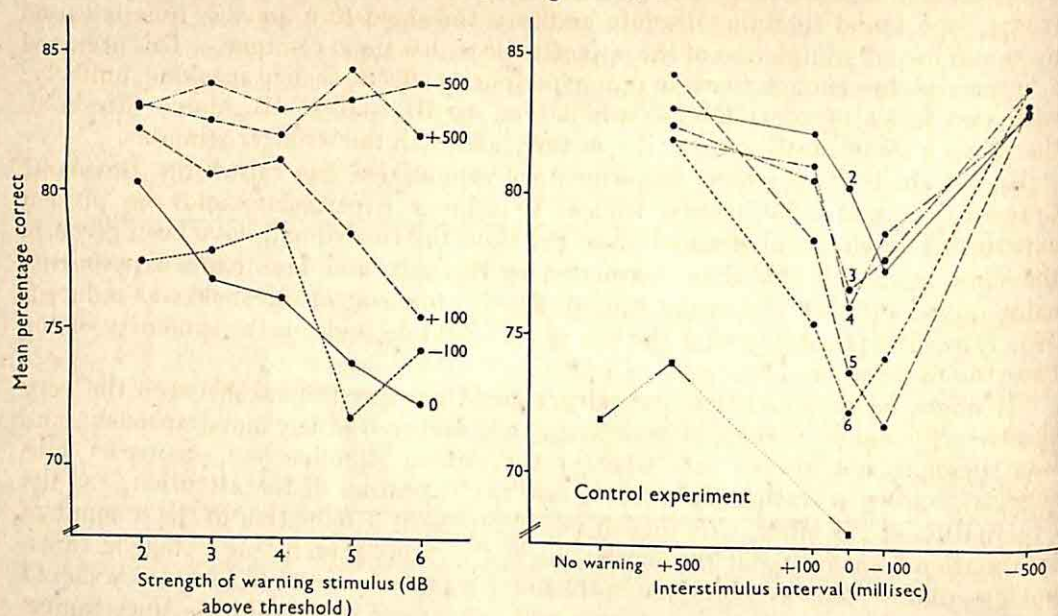
The mean percentage of correct responses was calculated in a similar way for the five different warning strengths, after pooling the data for all five interstimulus intervals. In Figure 2 (b) threshold curves are shown for the responses with the two weakest and the two strongest warning strengths (i.e. +2 to 3 dB. and +5 to 6 dB. above warning threshold). The threshold with the stronger warnings is higher than that with the weaker warnings.

The significance of this difference was examined by calculating the difference in the percentage of stimuli felt by each of the ten subjects for the weak and strong warnings. This gave a mean drop of 2.76 per cent. in the number felt with the two strongest warnings ($p < 0.01$). The mean change in threshold was also calculated by probit analysis, threshold curves being plotted for each subject on probit paper. This gave a mean difference of 0.335 dB. ($p < 0.01$). It is worth pointing out that both these significant differences are calculated on data for all five intervals combined, on which the original analysis of variance had only given warning strength as a factor significant at the 5 per cent. level.

The earlier analysis showed warning strength as having a more marked effect when those runs with the shorter interstimulus intervals were considered alone. Figure 3 illustrates the interaction between warning strength and interval with the data plotted in two different ways. At the 500 millisecc. intervals there appears to be no differential effect of warning strength on the mean percentage correct, but this percentage is progressively reduced by increasing warning strength at the three shorter intervals (Fig. 3 (a)).

Figure 3 (b) shows the interval effect more clearly. These results differ from those of Howarth and Treisman in the direction in which the threshold level is changed by the warning stimulus, but the interval effect resembles theirs in being at its greatest with a simultaneous warning and falling off with increasing interval in roughly the same way, whether the "warning" precedes or follows the "critical" stimulus.

FIGURE 3 a and b



Interaction between strength of warning stimulus and interstimulus interval. Figure 3 (a) shows the mean percentage correct for each interval separately. The warning strength exerts an effect at the three shorter intervals. Figure 3 (b) is a plot of the same data expressed in terms of interstimulus interval for each warning strength separately. The threshold is higher at the shorter intervals. The data for all five warning strengths in the control experiment has been pooled and is shown in Figure 3 (b). It will be noted that the control experiment here is somewhat higher than in the main experiment so that fewer stimuli have been seen in all three conditions. The "no warning" condition is significantly different from the simultaneous condition ($p < 0.05$), but not from the +500 millisecond condition.

DISCUSSION

These results, like those of Crawford (1947), do not appear to be explicable in terms of the "marker" hypothesis adopted by Howarth and Treisman. The effect of the "warning" or "conditioning" stimulus, though retroactive, is to raise the threshold for the test stimulus. The stimulus is therefore best seen when the subject's time uncertainty is at its greatest. In Crawford's experiment, both stimuli were fed into the same eye and regularly repeated every 7 sec., so that conditions are not perhaps strictly comparable; but it is clear that the effect on threshold of the conditioning stimulus (determined by the method of limits) was progressively reduced as the interval increased from 20 to 100 millisecond. The present experiments confirm the existence of such a retroactive effect, raising the threshold, in a different sense modality and suggest that it may be a quite generalized characteristic of the interaction of stimuli in the higher centres responsible for perception.

The additional finding that the magnitude of the effect depends on warning strength shows that it is not the mere perception or imperception of the warning stimulus which determines its effect on the critical threshold. (Though only 2 dB. above the initial threshold, even the weakest warning was only missed on one or two isolated occasions during the whole experiment.) This was considered to be unlikely of a suprathreshold stimulus which was simply acting as a marker, but to point rather to some active excitatory or inhibitory process of interaction between the two stimuli

in the central mechanisms. The present results can be compared to those of Ingham (1957), who found that the absolute auditory threshold to a 400 c/s. tone is raised by simultaneous stimulation of the opposite ear with a 1,000 c/s. tone. This occurred at intensities low enough to make purely peripheral effects, such as masking, unlikely; with two levels of contralateral stimulation, 10 dB. and 30 dB. above threshold, there was a significantly greater rise in threshold with the stronger stimulus.

In all three cases where supplementary stimulation has raised the threshold, Crawford's visual experiments, Ingham's auditory experiments and the present experiment on electrical stimulation of the skin, the two stimuli have been given in the same modality; this is in distinction to Howarth and Treisman's experiments using mixed auditory and visual stimuli, where a lowering of threshold was induced. This raises the possibility that the rise in threshold depends on the similarity of the two stimuli being used.*

It might be supposed that the subject could not discriminate between the very similar critical and warning stimuli when they occurred nearly simultaneously, and was therefore not always sure whether the critical stimulus had occurred. This suggests confusion, rather than distraction or "capturing of the attention," as the explanation of the effect. Confusion would represent a reduction in the amount of information transmitted at the shorter intervals, rather than a true change in threshold, so that as well as a reduction in the number of critical stimuli seen, there should be an increase in the false positive rate and an increase in the subject's uncertainty. But there is no evidence that either of these last two effects goes hand in hand with the reduction in the number of critical stimuli seen in the present results. It is also difficult to see how much confusion can occur when the critical stimulus occurs

TABLE V
INCIDENCE OF DOUBTFUL AND CORRECT RESPONSES TO CRITICAL STIMULI
WITH DIFFERENT INTERSTIMULUS INTERVALS

	Interstimulus interval (millisec.)					Overall mean
	+500	+100	0	-100	-500	
No. doubtful	132	146	131	133	88	126.0
No. correct	990	918	863	862	1028	932.2
Doubtful correct ..	13.3%	15.9%	15.2%	15.4%	8.6%	13.51%

$$\chi^2 = 27.79. \text{ d.f. } 4. \text{ } p < 0.001.$$

$$\chi^2 \text{ (excluding } -500 \text{ millisec. interval)} = 2.34. \text{ d.f. } 3. \text{ } p 0.5.$$

* While this paper has been in press, we have found a most interesting and important paper by Piéron and Segal (*J. Neurophysiol.*, 2, 178-91, 1939), demonstrating retroactive facilitation occurring on double stimulation of the middle finger with condenser discharges. In their experiments both stimuli were delivered through the same electrodes. They were able to show that two consecutive stimuli, separated by intervals of between 20 to 400 millisec., became perceptible at voltages which were invariably subthreshold for a single shock. This effect involved retroactive facilitation, as both stimuli were perceived with the appropriate interval between them; simple facilitation would have led to the perception only of the second shock of the pair. This finding appears to dispose of the possibility that facilitation is related to the use of stimuli in mixed modalities; and shows that the threshold will not necessarily be raised if the two stimuli are similar.

100 millise. before or after the warning. The false positive rate, which is in any case low, shows no particular tendency to be higher with the simultaneous warning (Table I). The subject's uncertainty has been assessed by calculating the ratio of doubtful to correct responses when critical stimuli are being presented. Comparing this ratio for the five intervals (Table V), one significant difference emerges. The subject was definitely less doubtful with the warning coming 500 millise. after the critical stimulus. The differences between the other four intervals were insignificant, but there does appear to be a tendency to more doubtfulness for the runs with the three shorter warning intervals. However, comparing the same ratio for the five warning strengths (Table VI), no significant differences emerge, and there is (if anything) a rather lower value for the strongest warning, with which fewest critical stimuli are seen. The same lack of significance is shown by a similar comparison of warning strengths on data for the three shorter intervals alone. These results suggest that confusion can be ruled out as an explanation of the warning effect.

TABLE VI
INCIDENCE OF DOUBTFUL AND CORRECT RESPONSES TO CRITICAL STIMULI
WITH DIFFERENT STRENGTHS OF WARNING STIMULUS

	Strength of warning stimulus (dB. above threshold)					
	2	3	4	5	6	Overall mean
No. doubtful	145	134	118	128	105	126.0
No. correct	954	946	946	909	906	932.2
Doubtful correct ..	15.2%	14.2%	12.5%	14.1%	11.6%	13.51%

$$\chi^2 = 5.60. \text{ d.f. } 4. \text{ } p > 0.2.$$

Another possibility is that the "warning" stimulus simply distracts the subject from the critical stimulus by "capturing his attention" without necessarily confusing him. It is important here to make clear what can and cannot be implied by this hypothesis. The retroactive effect of the warning precludes any low-level attenuation or switching of the input, but may still leave the question of a high-level switch open. Broadbent (1957) in his model of the perceptual mechanism, accepts that a more intense stimulus will tend to "capture" the single information channel of a central mechanism which is the supposed seat of attention, and, having done so, will render it temporarily inaccessible to other stimuli. He cites some results of Berlyne (1950), showing that, other things being equal, a subject will respond to a stronger visual stimulus significantly more often than to a weaker one, if he is instructed to respond to any one of a number of stimuli occurring simultaneously. In the present experiment the warning stimulus is usually stronger than the critical stimulus and exerts most effect when at its greatest strength. There is no reason why the warning should not be effective coming 100 millise. later than the critical stimulus on this view, provided the latter is still waiting to enter the central channel and be perceived.

In this explanation the effectiveness of the warning in reducing the number of critical stimuli perceived depends essentially on its capacity to capture the conscious attention of the subject. It should clearly be accepted only with caution, for it does not explain why the opposite results were obtained in Howarth and Treisman's situation, unless the warning is in some way less demanding in their situation.

Evidence from past experiments on the effect of altering the set of the subject to favour distraction is somewhat indefinite and conflicting. Ingham (1957) found no significant difference in the rise of auditory threshold when his subjects were also made to report on small changes in frequency of the contralateral tone. Brogden and his co-workers (Gregg and Brogden, 1952; Thomson, Voss and Brogden, 1958) obtained a lowering of the auditory threshold on giving a simultaneous visual warning, provided the subject was not especially required to attend to it; but when instructed to attend and report on it as well, the results were not so consistent, showing a less marked fall in threshold or, at near threshold strengths, a rise. A particularly interesting finding in their results is that with different strengths of visual warning, the lowering of threshold in the "uninstructed" condition was significantly greater with the stronger warning. Unfortunately, their stimuli were always simultaneous and yield no information about any interval effects, but these results suggest that even with a lowering of threshold, warning strength is significant. This seems to be evidence in favour of the theory of central interaction between the stimuli rather than the "marker" theory, even in explaining those situations where a fall in threshold is induced.

Our thanks are due to Mrs. M. O. Kerr for her invaluable help in the statistical design of the experiment, and to Dr. E. A. Carmichael for his interest and encouragement.

REFERENCES

- ADRIAN, E. D. (1958). The control of the nervous system by the sense organs. *Archiv. Psychiat. Nervenkrankh.*, **196**, 482-93.
- ALAJOUANINE TH., SCHERRER, J., BARBIZET, J., CALVET, J., and VERLEY, R. (1958). Potentiels évoqués corticaux chez des sujets atteints de troubles somesthésiques. *Revue neurol.*, **98**, 757-62.
- BERLYNE, D. E. (1950). Stimulus intensity and attention in relation to learning theory. *Quart. J. exp. Psychol.*, **2**, 71-5.
- BRAZIER, M. A. B. (1954). The action of anaesthetics on the nervous system. In Delafresnaye, J. F. (Ed.). *Brain Mechanisms and Consciousness*. Oxford.
- BROADBENT, D. E. (1957). A mechanical model for human attention and immediate memory. *Psychol. Rev.*, **64**, 205-15.
- CRAWFORD, B. H. (1947). Visual adaptation in relation to brief conditioning stimuli. *Proc. roy. Soc., B*, **134**, 283-302.
- DAWSON, G. D. (1958). The central control of sensory inflow. *Proc. R. Soc. Med.*, **51**, 531-5.
- GIBLIN, D. R. (1960). The effects of lesions of the nervous system on cerebral responses to peripheral nerve stimulation. *Electroenceph. clin. Neurophysiol.*, **12**, 262.
- GREGG, L. W., and BROGDEN, W. J. (1952). The effect of simultaneous visual stimulation on absolute auditory sensitivity. *J. exp. Psychol.*, **43**, 179-86.
- HOWARTH, C. I., and TREISMAN, M. (1958). The effect of warning interval on the electric phosphene and auditory thresholds. *Quart. J. exp. Psychol.*, **10**, 130-41.
- INGHAM, J. G. (1957). The effect upon monaural sensitivity of continuous stimulation of the opposite ear. *Quart. J. exp. Psychol.*, **9**, 52-60.
- THOMSON, R. F., VOSS, J. F., and BROGDEN, W. J. (1958). Effect of brightness of simultaneous visual stimulation on absolute auditory sensitivity. *J. exp. Psychol.*, **55**, 45-50.
- TREISMAN, M., and HOWARTH, C. I. (1959). Changes in threshold level produced by a signal preceding or following the threshold stimulus. *Quart. J. exp. Psychol.*, **11**, 129-42.
- WOODROW, H. (1930). The reproduction of temporal intervals. *J. exp. Psychol.*, **13**, 473-99.

LOWERING OF AN AUDITORY THRESHOLD BY A NEAR THRESHOLD WARNING SIGNAL

BY

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Following previous experiments on the effect of suprathreshold "warning" signals in lowering the threshold for stimuli in another modality, we have used visual "warning" signals which are themselves within the threshold range. These near threshold "warnings" appear to act in exactly the same way as suprathreshold "warnings" provided they are seen. Very weak "warnings" have a reduced effect, probably because they are not always seen. The experiment is of interest in that it supports the theory that the effect is due to the "warning" signal reducing the subject's uncertainty about when the threshold stimulus will occur, and secondly because it indicates that under these conditions at least, the subject can attend effectively to two simultaneous near-threshold stimuli.

INTRODUCTION

Recent work by the present authors (Howarth and Treisman, 1958 *a, b*; Treisman and Howarth, 1959) has shown that the auditory threshold can be lowered by a simultaneous visual stimulus which is itself well above threshold. It can also be lowered, though less effectively, by visual stimuli which occur up to 9 sec. before the auditory stimulus, or up to $\frac{1}{2}$ sec. after it, provided the subject is informed beforehand what will be the time interval between the threshold stimulus and the warning signal. The effect disappears if the subject does not know what interval to expect between them. Because of this we suggested that the effect was due to the warning signal reducing the subject's uncertainty about when to expect the threshold stimulus. The lowering of the threshold was not accompanied by any increase in the subject's responses to zero stimuli (the false alarm rate) and we thought this was because the effective "noise" level in the system was lower, the more accurately the subject knew when to expect the threshold stimulus.

One of the predictions one might make from this hypothesis is that the intensity of the warning signal should not have very much effect, provided it is big enough to be seen. The present experiment sets out to test this prediction. It is only fair to add that there are reasons why one might expect threshold warning signals to raise, rather than lower the threshold for the auditory stimulus. Pauli (1924) has shown that the threshold for each of two simultaneously presented stimuli is higher than when each is presented separately. This has been considered evidence that we cannot attend simultaneously and effectively to two threshold stimuli. If our warning signal is to be effective it must in some sense be attended to. If such an interference between the two stimuli were to occur it should be very much less if the two stimuli are not quite simultaneous, so we also used a 200 millisecc. interval between the warning and the threshold stimulus, to be compared with the no warning and the simultaneous conditions.

APPARATUS

The subject was in an artificially illuminated room, separated from the experimenter and the timing and recording apparatus. A white cardboard screen, 50 cm. $\times$ 60 cm. in size, was placed 1 m. in front of him. At the centre of the screen was a fixation cross, and 25 cm. to the right of this was a neon bulb shielded by a sheet of white paper, which

provided the prewarning. The auditory stimulus was a 40 millisecon. increment in intensity of a constant 500 c.p.s. tone at 60 to 70 dB. S.L. from a Muirhead Audio-oscillator, which was amplified and came to the subject through Brown type-K moving coil earphones. It was continuously present throughout the session. The visual stimulus was provided by an electronic generator passing a constant current 10 millisecon. pulse through a neon bulb which was focussed through a lens on to the centre of the fixation cross to give a square spot of light, 4 mm. $\times$ 4 mm., subtending about 14 min. of arc at the subject's eye. The intensity of the light spot could be varied by alternating the strength of the constant current. Electronic timing units determined the time intervals. The brightness of the walls of the room was 0.20 foot lamberts, the screen was 3.5 foot lamberts and the prewarning light 10 foot lamberts. The strength of the stimulus was determined separately for each subject as described below. The apparatus has been fully described elsewhere (Howarth and Treisman, 1958 a).

Three experienced subjects, including one of us (C. I. H.), were used. They were research psychologists and had had practice in similar experimental situations. Two did four sessions each, the third (J. H.) did eight in two groups of four separated by rather a long interval so that for the purpose of analysis her data were treated as if they came from two different subjects.

PROCEDURE

Each trial began with the prewarning neon lighting up. (It stayed on until the subject had responded.) This was followed, after an interval varying randomly between 2 and 4 sec., by the auditory "pip"—a 40-millisecon. increment in the intensity of the background tone. The subject responded Yes or No by depressing one of two tapping keys. The auditory stimulus could take one of four values, equally spaced on a decibel scale, and zero. The 10 millisecon. pulse of light was simultaneous with the pip in the "Simultaneous" (Si) condition, and preceded it by 200 millisecon. in the "Warning First" (Wf) condition. The light stimulus had three values—"Strong," "Weak" and "Zero."

At the beginning of each session, descending series of light, and of auditory stimuli were given to the subject to determine approximately his threshold levels for each, and on the basis of these, values were chosen for the four intensities of auditory stimulation, and two values for the light stimulus intended to be approximately at strengths which would evoke 50 per cent. and 95 to 100 per cent. frequency of seeing.

A series of 15 trials presented the five auditory stimulus strengths, and the three light strengths, so that each combination occurred once, in a random order different for each series. Ten series under the "Simultaneous" condition alternated regularly with 10 under the "Warning First" condition, so that in a session, 300 responses to the auditory stimulus were given. Furthermore, at the beginning of each session, and also at the end, there were 40 trials in which only the light stimulus occurred, taking one of the two values Weak or Strong randomly, and the subject responded to the light. This allowed a check on the effective strengths of the warning stimuli.

Each session lasted about 1½ hr.

The experimental instructions were that the subject should fixate on the cross when the prewarning lit up, as a warning light spot might occur with, or 200 millisecon. before, the pip. *The subject was always told before each series, what time interval to expect between the two stimuli* and was given an initial trial at the beginning of each series with high values of both stimuli, which was not used in calculating the results.

RESULTS

Each set of four sessions was combined, to give probabilities of response for each condition and each intensity of stimulus. A probit analysis (Finney, 1952) was performed on these for each visual warning strength under each condition, giving 24 thresholds in all (four subjects, six conditions). Before the probit analysis was done the probabilities of response were corrected in the usual way for false positive responses. This is important since in this experiment, unlike our previous experiments, the lowering of threshold was accompanied by an increased false positive rate. If these false positive responses had been due to guessing on the part of the subject they could have produced an apparent change in threshold. Since we applied the

"guessing" correction such an explanation cannot be given for the changes in threshold which we find. The data from the individual subjects all conformed to a fairly consistent pattern, but it was decided to pool the data from the different subjects for the sake of clarity. Pooling of data derived from a probit analysis can be done in a variety of ways. We could have simply averaged the thresholds for the different subjects. A better estimate of the mean threshold would have been obtained by weighting each estimate of the threshold by the reciprocal of its variance. Curiously, however, this does not give the best estimate of the differences between these thresholds. This is done by estimating for each subject the difference between any two thresholds together with an estimate of the variance of that difference. The best estimate of the differences is now obtained by weighting each difference by the reciprocal of its variance and then calculating the mean of the differences. An odd feature of these estimates is that they are not necessarily additive in the sense that the difference between conditions *a* and *c* need not be equal to the difference between *a* and *b* plus the difference between *b* and *c*. Although this is rather confusing we decided to use these best estimates of the differences rather than unweighted differences (which would have been additive) since the significance levels are easier to calculate this way, and anyhow the overall picture is very much the same as for the unweighted means. The results we show in Table I.

The Zero thresholds, which are determined from the trials on which the light stimulus does not occur, provide a check for any general effect of the experimental instructions during the Warning First or Simultaneous series. As there is no significant differences between them, experimental "set" does not account for differences between the two conditions. The mean of the eight Zero thresholds is 0.485 dB.

The percentages of false positives for all subjects combined, are given in Table II. Each is derived from 160 Zero stimuli.

The differences between slopes were compared in the same way as the differences between thresholds in Table I. Only one of the 15 comparisons (Wf Zero-Si Zero) was significant at the 5 per cent. level.

TABLE I
MEAN DIFFERENCES BETWEEN THRESHOLDS

Comparison	Difference (dB.)	S.E.	<i>t</i>	Significance
<i>Warning First</i>				
Zero-Weak	0.045	0.0113	3.98	$p < 0.001$
Weak-Strong	0.000	0.0109	0.00	$p > 0.9$
Zero-Strong	0.050	0.0122	4.10	$p < 0.001$
<i>Simultaneous</i>				
Zero-Weak	0.047	0.0110	4.27	$p < 0.001$
Weak-Strong	0.034	0.0115	2.96	$p < 0.01$
Zero-Strong	0.074	0.0124	5.97	$p < 0.001$
Wf Zero-Si Zero	-0.001	0.0117	0.09	$p > 0.9$
Wf Weak-Si Weak	0.000	0.0104	0.00	$p > 0.9$
Wf Strong-Si Strong	0.042	0.0125	3.36	$p < 0.001$

The correlation between threshold and slope was $r = -0.406$, which is not very significant ($0.1 > p > 0.05$). The correlation between threshold and false positive rate was $r = -0.268$, which again is not significant ($p > 0.2$).

The percentages of response to the visual stimuli at the beginning of sessions in the four sets of results were:—

Weak: 48.8, 43.8, 51.3, 71.3.

Strong: 88.8, 96.3, 98.8, 98.8.

At the end of sessions they were:—

Weak: 47.5, 50.0, 27.5, 45.0.

Strong: 93.8, 96.3, 85.0, 95.0.

For all together response rates were: Weak, 48.3; Strong, 94.1.

TABLE II
FALSE POSITIVE RATES (PERCENTAGES)

<i>Warning First</i>			<i>Simultaneous</i>		
<i>Zero</i>	<i>Weak</i>	<i>Strong</i>	<i>Zero</i>	<i>Weak</i>	<i>Strong</i>
1.9	3.1	8.1	0.6	3.1	6.3

In our earlier experiments strong warning lights lowered the threshold by about 0.1 dB. The stronger of our two near threshold "warning" signals lowered the threshold by 0.074 dB. The "warning" signal in the present experiment was about one-hundredth the intensity of the warning signal in the earlier experiments so that even if intensity of warning has some effect it must necessarily be very small, especially as there were inevitably small differences between the two experiments which themselves could account for the difference. We had hoped that the comparison between the two intensities of warning which we used would have provided clearer evidence for interference between the two threshold stimuli. However, the weaker of our two warnings is, in the Simultaneous condition, less effective than the stronger one, but only by an amount fully accounted for by the fact that it is only seen on about half the trials. The effect of the Weak warning is about half that for the Strong warning. This is exactly what would be expected if the warning had a full strength effect provided it were seen, but the weaker warning was only seen half as often as the stronger one.

In the Warning First condition the threshold with the stronger warning is significantly higher than for the Simultaneous condition. This is what is found for stimuli well above threshold and is further evidence against the hypothesis that we cannot attend to two simultaneous, near threshold stimuli. However, for the weaker warning signal, the situation is rather different. The threshold is about the same for the Warning First condition and the Simultaneous condition. This is contrary to our findings with strong warning signals and could be accounted for in terms of some kind of interference between the visual and auditory threshold stimuli when they occur together. However, it is hard to see why a visual stimulus intensity which is seen on 94 per cent. of trials should not interfere with the auditory stimulus, while one which is seen on 48 per cent. of trials does. One would expect both to require processing by the threshold mechanism and if the stronger of our warning signals were by-passing the threshold mechanism, one would expect a bigger difference between the Strong and the Weak warning signals in the Simultaneous condition.

DISCUSSION

The results of these experiments are of interest for two reasons. Firstly they show that the intensity of warning signal had very little effect on the amount by which the threshold is lowered in this kind of experiment. Our earlier experiments with strong warning stimuli showed a maximum lowering of the threshold of about 0.1 dB. while in the present experiment the threshold is lowered 0.074 dB. for the stronger Simultaneous warning. It cannot be too strongly emphasized that the effects of warning in all our experiments are extremely small, of the same order as the variability of the threshold measurements. This is, in itself, evidence for the theory that it is due to a change in the effective "noise" level in the system. The fact that the intensity of the warning signal has so little effect is evidence for the theory that the warning acts simply as a "marker" (a very useful word which has been suggested to us by Dr. Halliday) which reduces the subjects uncertainty about just when the stimulus is to occur. It is true that in our present experiment the weaker warning signal had only about half the effect of the stronger warning, but since at the lower intensity the warning light was seen on only 48 per cent. of the occasions when it was presented alone, before and after the main body of the experiment, this reduced effect is not necessarily a direct result of the lower intensity of the warning. The result could be equally well accounted for by assuming that the warning signal had a maximum effect provided it was seen. The two strengths of warning had exactly the same effect in the Warning First condition, but we feel unable to draw any very strong conclusions from this beyond the fact that here also the effect of intensity of warning is small.

While the present experiment provides some evidence in favour of our "marker" theory of the warning effect, the theory has not yet been shown to account for all the lowering of threshold in the simultaneous condition. There may be some simple facilitatory or summative effect in this condition. In an earlier paper in which we used warning intervals between 1 and 9 sec. we showed that over this range of time intervals no simple facilitatory effect was occurring, because when the subject did not know what interval to expect between the "warning" signal and the threshold stimulus, no lowering of the threshold was produced. Unfortunately we have not yet been able to do the analogous control experiment for near simultaneous "warnings."

The second point of interest in these data is that just because the effect of warning strength is so slight, there is no evidence in these experiments for any interference between the two simultaneous threshold stimuli. This idea of interference between stimuli has been used in one form or another in relation to at least three different kinds of experiment. Pauli (1924) has shown that when subjects are asked to count dots in a tachistoscopic visual display and at the same time report on a threshold tactile stimulus, they do much worse on both tasks than they do when performing them separately. This has been held to indicate that "simultaneous performance of two attentive acts of cognition did not often, if ever occur" (Woodworth and Schlosberg, p. 90). Interference of different, although possibly related kinds are postulated by Welford (1952) and others to account for the longer reaction times which are found to the second of two successive stimuli, and by Broadbent (1957) to account for the effects observed in experiments on listening to two speech messages at the same time. In both kinds of experiment there is evidence that the response to one of the stimuli or messages is delayed until the first has been dealt with. If the trace of a near-threshold stimulus decays appreciably during this delay then results such as Pauli's could be accounted for in the same way. Perhaps, the best evidence for the rapid decay of such a trace is found in our recent experiments on the effect of "warning"

signals presented after the threshold stimulus (Howarth and Treisman, 1958 *b*; Treisman and Howarth, 1959). If these experiments are accepted as evidence of the rate at which such a trace would decay, it is clear that an appreciable loss would be produced by delays of the magnitude suggested by Welford, although in Broadbent's experiments much longer delays are possible without loss of signals which are well above threshold.

Why is there no apparent interference between the two threshold stimuli in our present experiment? There seem to be several possible explanations. Our subjects made only one response to the two stimuli and possibly whatever interference occurs is in the response side of the sensory motor chain. Alternatively one might say that the information in the message was unaffected by the presence of the warning signal and hence, if the interference is of the kind caused by the attempt to transmit too much information along a communication channel of limited capacity, one would not expect interference in such an experiment. Two other features of our experimental situation are worth mentioning. Firstly the warning signal and the threshold stimulus were in different sense modalities. In many of the experiments showing interference between stimuli, they were in the same modality, which may increase the possibilities for interference. Secondly, our subjects adopted a relaxed attitude towards the warning signal, they were instructed not to try consciously to see it. If they had tried too hard to see it there may possibly have been more interference.

It should be pointed out that our experiment is not alone in showing no interference between two simultaneous threshold stimuli. There are many experiments in which two stimuli are given, but only one response required and in many of these no interference occurs. Examples of this type of experiment can be found in the comparison between monocular and binocular thresholds (Pirenne, 1943; Collier, 1954), monaural and binaural thresholds (Smith and Licklider, 1949), and the experiments of Loveless (1957) on the threshold for simultaneous and perfectly correlated, visual and auditory stimuli. In all these experiments the difference between the single and double stimulation can be largely accounted for as statistical summation. There is no evidence of refractoriness. Rather, the existence of statistical summation suggests that the two threshold decisions are taken separately and independently.

Experiments by Gregg and Brogden (1952) and by Thompson, Voss and Brogden (1958) seem to indicate that the absolute auditory threshold can be raised by a simultaneous visual stimulus when the subject has to respond to it, but may be lowered by it when he does not have to respond to it. The effects found by these authors are much bigger than in our experiment, suggesting that they have a different cause. In our opinion their experiments were insufficiently controlled for the effects of set, warning, practice, interference or intersensory facilitation to be disentangled. Nevertheless we feel that their final conclusion as summarized above is probably correct. The larger changes in threshold which Brogden *et al.* found could well be due to their subjects' expectations reinforcing the facilitatory, warning and interference effects, none of which are after all, very surprising (with the possible exception of the effect produced by a "warning" signal which comes after the threshold stimulus).

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REFERENCES

- BROADBENT, D. E. (1957). A mechanical model for human attention and immediate memory. *Psychol. Rev.*, **64**, 205-15.
- COLLIER, G. (1954). Probability of response and intertrial association as functions of monocular and binocular stimulation. *J. exp. Psychol.*, **47**, 75-83.
- FINNEY, D. J. (1952). *Probit Analysis*. London.
- GREGG, L. W., and BROGDEN, W. J. (1952). The effect of simultaneous visual stimulation on absolute auditory sensitivity. *J. exp. Psychol.*, **43**, 179-86.
- HOWARTH, C. I. and TREISMAN, M. (1958 a). The effect of warning intervals on the electric phosphene and auditory thresholds. *Quart. J. exp. Psychol.*, **10**, 130-41.
- HOWARTH, C. I., and TREISMAN, M. (1958 b). Lowering of an auditory threshold produced by a light signal occurring after the threshold stimulus. *Nature*, **182**, 1093-4.
- LOVELESS, N. E. (1957). Signal detection with simultaneous visual and auditory presentation. *F.P.R.C. 1027 Report to the Flying Personnel Research Committee, Air Ministry*.
- PAULI, R. (1924). Reported in WOODWORTH, R. S., and SCHLOSBERG, H. *Experimental Psychology* (1955). London.
- PIRENNE, N. J. (1943). Binocular and uniocular thresholds of vision, *Nature*, **152**, 698.
- SMITH, M. H., and LICKLIDER, J. C. R. (1949). Statistical bias in comparisons of monaural and binaural thresholds; binaural summation or binaural supplementation. *Psychol. Bull.*, **46**, 278-84.
- THOMPSON, R. F., VOSS, J. F., and BROGDEN, W. J. (1958). Effect of brightness of simultaneous visual stimulation on absolute auditory sensitivity. *J. exp. Psychol.*, **55**, 45-50.
- TREISMAN, M., and HOWARTH, C. I. (1959). Changes in threshold level produced by a signal preceding or following the threshold stimulus. *Quart. J. exp. Psychol.*, **11**, 129-42.
- WELFORD, A. T. (1952). The "psychological refractory period" and the timing of high-speed performance—a review and a theory. *Brit. J. Psychol.*, **43**, 2-19.

AN INVESTIGATION OF A SATIATION PROCESS IN THE REVERSIBLE PERSPECTIVE OF REVOLVING SKELETAL SHAPES

BY

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A technique for studying a satiation process is described, which is claimed to have many advantages over the older methods. A skeletal wire cube is rotated at 1 rev. per sec., and the time taken for the first apparent reversal of direction to occur is measured and called the "satiation period." It is a much longer period than the mean period of subsequent reversals. This is due to the fact that the situation is one of biased ambiguity; biased by the cues to depth and thus to the true perspective.

Using this measure we have shown:—

- (1) That the effect is predominantly central.
- (2) That after satiation has occurred, reversing the cube's direction of rotation induces a longer than normal satiation period.
- (3) There is no transfer of satiation to other types of static or mobile figure nor to other spatial locations.
- (4) Satiation time is largely independent of brightness and colour.
- (5) Köhler's theory of satiation is supported but it is given a neuronal basis rather than a basis in a non-neuronal, electrolytic type of cortical conduction.

INTRODUCTION

There has been considerable interest shown by experimental psychologists in the problem of reversible perspective, since Köhler's speculation that the brain processes responsible for the figural-after-effects are also responsible for the reversals of ambiguous figures.

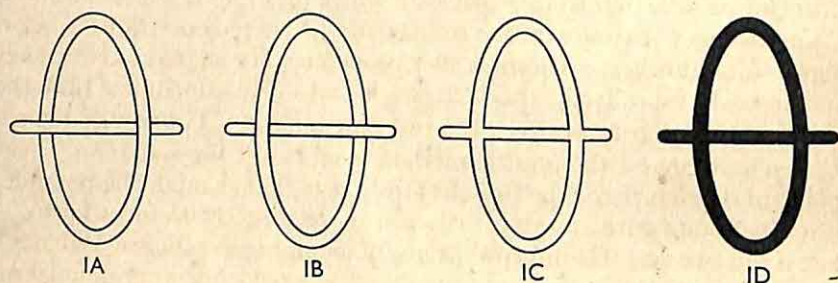
The 1944 paper of Köhler and Wallach stated, "According to the first hypothesis a specific figure process occurs whenever a figure appears in the visual field. And this process tends to block its own way if the figure remains for some time in the same location. The second hypothesis states that in reversible figures, the figure ground relationship will suddenly be reversed when the figure process has altered the medium beyond a critical degree." In support of his theory, Köhler (1939) showed that the rate of figure-ground reversal in a Rubin cross increases with long fixation.

Köhler's term "satiation" is used throughout this paper and the formal aspects of his theory are supported. However, as will be shown later, Köhler's specific physiological speculations regarding non-neuronal, electrotonic spread of cortical potentials do not receive support. We attach no precise physiological significance to the term "satiation" beyond taking it to mean an auto-inhibitory process occurring at specific synapses in the central nervous system as a result of activity set up there by specific sensory events.

It is of historical interest to note that McDougall in an unacknowledged paper, written as early as 1906, stated a view formally equivalent to Köhler's. He wrote, "Reversible perspective is a special case of a general psychic characteristic, viz. the rapid fatiguability manifested in all the higher levels of consciousness, due to rapid changes in synaptic resistance." Like Köhler, he found that the rate of reversal of a Rubin cross increases with long fixation.

Others have reported evidence in support of Köhler's theory in recent years. Fisichelli (1947) used a moving Lissajous figure on a cathode-ray tube and found that the rate of reversal was enhanced by increasing the complexity of the pattern, the inspection time, or the speed at which the pattern moved. He suggested that the enhancement was due to a concomitant increase in the rate at which cells were stimulated and that this supported Köhler's theory. Hochberg (1950) used the Rubin cross and found, "Prior satiation of the visual region upon which one of the two alternative figures of an ambiguous test pattern is later presented results in the relative decrease in perception-as-figure of the portion of the test pattern subsequently coincident with that region." This supports Köhler's theory. The black-white relationships had to be kept the same for this effect to show; this "directional" effect is opposed to Köhler's view.

FIGURE 1



Figures 1 A—D

Carlson (1953) used the figures depicted in Figures 1a-d. Figure 1c when viewed steadily, undergoes alternations as one edge or the other apparently comes forward.

Twenty-six subjects were shown one of the Figures 1a or 1b for 15 sec. Figure 1c or 1d was then substituted for 1a or 1b and the subject asked to report on which of the two ambiguous possibilities was then present. He found that, "A given alternative tends to appear first least often when it has been previously satiated, more often when neither alternative has been satiated, and most often when the opposite alternative has been satiated." He allowed that suggestion may have been a major factor in causing his results. There is another possibility, however, which Carlson does not mention. After inspecting Figure 1a or 1b there would result an after-image and this after-image would not be symmetrical at the points of intersection of the ellipse and rod. It may have been this asymmetrical after-image rather than any satiation effects which caused the subsequent differential effects in the Figures 1c and 1d.

Our own experiments overcome this difficulty (see experiment series C). Carlson also used the Boring wife-and-mother-in-law figure in which the reversal involves a change in meaning but not in perspective. One version was designed so that "mother-in-law" could be seen and one so that "wife" could be seen. Here he found that previous experience of one alternative in this way tended to favour that same alternative when an ambiguous figure was shown. This is the converse of the result with the perspective figure, but he argued that here meaning is the main factor and the result is due to expectancy or set. This strengthens the argument that the results of the first experiment were not due to expectancy.

Brown (1955) used as his pattern a circle of upright rotating pins, casting a silhouette on a flat screen. He, too, found that the reversal rate of apparent perspective increases with inspection time. The change in perspective was accompanied

by an apparent change in the direction of rotation of the pins. Brown claimed that this made the perspective changes easier to judge, and this is confirmed by our own observations.

Adams (1954) claimed to support Köhler's theory. She had her subjects prefixate a stationary $5\frac{1}{2}$ -in. skeletal cube monocularly, at 28 in. distant until it reversed its perspective. The eyes were then covered for 2 sec. and when the cube was exposed again to the same eye or the other eye the subjects reported whether the cube was in its reversed or correct perspective. It was found that the preliminary fixation lead to a greater number of reversals in the subsequent exposure as compared with the initial exposure.

Finally, Cohen (1959) investigated some points of similarity between reversible figures and figural-after-effects. Using flat drawings and the increase in reversal rate with time as an indicator of satiation, he showed among other things that (a) there is transfer of satiation from a black-on-white figure to a white-on-black figure; (b) there is no transfer spatially on the retina; and (c) there is no transfer between one type of figure and another. The first two properties are shared between reversible figures and figural-after-effects, the last one is not. He concluded that there was a large measure of similarity between the two phenomena. The results reported here confirm Cohen's findings although the method used is not the same.

The point of departure for the present study was that it might be possible to find a type of ambiguous figure showing satiation more directly than hitherto. It was argued that if the two possible interpretations of an ambiguous figure were not equally probable in experience, then the less probable one would not appear until after the satiation of the more probable one. This delay should provide a better measure of satiation than the reversal rate used by others.

To this end a three-dimensional cube was made, this naturally had a bias in favour of the true perspective. As expected, it now took some time before the alternative, inverted, false perspective came in experience, but this was only a few seconds.

It was then accidentally found that if the cube was rotated slowly the perspective did not reverse for many seconds. The spinning had evidently increased the bias in favour of the true perspective. This could be because the movement gave added cues to depth and/or because the movement prevents any one group of cells being continuously stimulated. Langdon (1951) has shown how shape constancy is increased when any object rotates, which supports the first possibility. Evidence will be given later (experiment C3) in support of the second possibility also.

Now that the cube was rotating, any change in perspective incurred a change in the apparent direction of rotation as was the case in Brown's study (1955). This made the perspective reversals all the more easy to see.

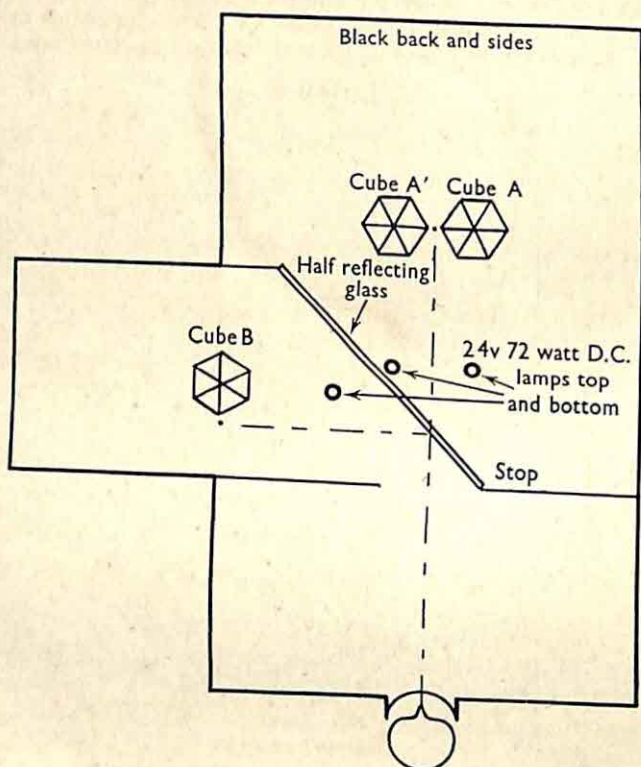
A speed of one revolution per sec. was finally chosen for further work, with rotation about a vertical axis.

A remarkable phenomenon was then noted. On closing one eye about 5 sec. after starting to look at the cube, there was an immediate apparent reversal; when the eye was again opened the cube returned to its true direction and perspective. This effect is very compelling. By closing one eye of the subject we were suddenly cutting out the stereoscopic cues to depth and thus reducing the bias constraining him to see the true perspective. We soon found that any means of suddenly reducing depth cues, had the same effect. We tried defocusing lenses, a prism before one eye, and suddenly taking the cube further away. In this situation there must be a latent degree of satiation present insufficient to cause reversal with two eyes open but immediately more than enough when one eye is closed.



Thus the period before the first reversal, called here the *satiation period*, is least for a flat cube drawing, longer for a stationary solid cube, or a one-eyed view of a rotating cube and longer still for binocular viewing of a solid rotating cube. As the bias increases in favour of one alternative then the satiation time also increases. One progresses from a situation of unstable equilibrium to one of relatively stable equilibrium. It is as if, in the biased situation, the satiation process has work to do in overcoming the resistance offered by the "cues" of true perspective. Donahue and Griffiths (1931) reached a similar conclusion by comparing the reversal rates of

FIGURE 2



various flat ambiguous objects where the alternative was more clearly defined in some of the figures. We have found in all our subjects that attitude does not influence the satiation time—but when the cube has once reversed it is in a satiated state of unstable equilibrium. Attention and attitude now have some effect as they do with the equally unstable, flat, Necker cube (Flugel, 1913). Thus it is felt that the technique of using a solid rotating figure and recording the time to the first reversal gives many advantages for the study of satiation processes, some of which are:

- (1) That it provides a quick, direct and easy measure, the "*satiation period*."
- (2) No estimates of extent are necessary on the part of the subject as in the figural-after-effects. Factors of attitude and suggestion are at a minimum.
- (3) The effect is present in all normal people apart from those without stereoscopic vision. Incidentally we screened all our subjects on the Howard-Dolman stereoscopic acuity test. Two subjects lacking stereoscopic vision showed no satiation time longer than a few seconds, that is, no longer than the satiation time for normal monocular viewing.

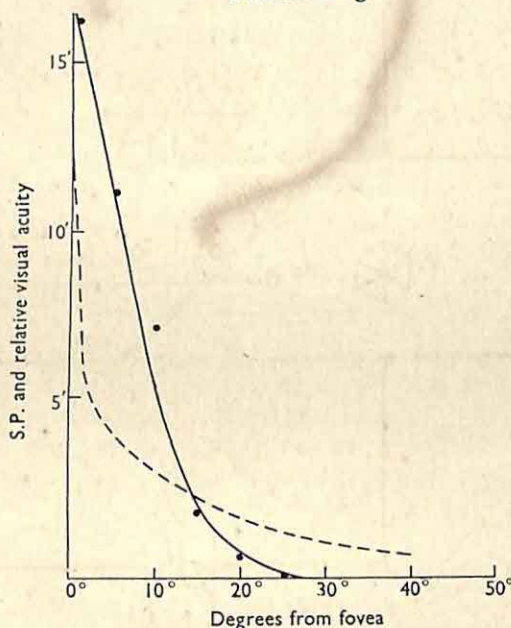
- (4) The technique is advantageous because it eliminates any change in the stimulus corresponding to the apparent reversal.
- (5) Central factors must be predominant, for this is the only way to account for the much longer satiation period with binocular viewing. There is no convincing proof that figural-after-effects are central. Binocular transfer of figural-after-effects is usually given as proof of this but as McEwen (1958) and Day (1958) have argued, this is not conclusive.

GENERAL EXPERIMENTAL METHOD

Figure 2 gives the general outlay of the apparatus.

The cubes had 4-in. sides and were made by brazing $\frac{1}{16}$ -in. wire. They were rotated at 1 rev. per sec., about a vertical diagonal axis by silent clockwork motors. They were placed in a large, black-coated box and viewed from an aperture at a distance of 40 in.

FIGURE 3



Graph showing the variation of the satiation period with increasing retinal eccentricity (Solid line) based on ten readings at each angle. The broken line shows the regional variation of relative visual acuity (after Duke-Elder 1932).

On the aperture were two manually operated shutters, so that either eye could be covered. Illumination was by two 24-volt, 72-watt D.C. car headlamp bulbs to each cube, placed above and below out of sight and arranged so as to light up the white-painted cubes and leave the background black. Light intensities were controlled and matched for the two cubes, etc., by means of a photocell placed in front of each. The speeds were synchronized and standardized with the aid of a stroboscope.

The subjects recorded the time of reversal by pressing a key, this was timed on a time-event recorder to the nearest second. Fixation was maintained on a small white bead suspended to the side and $3\frac{1}{2}$ in. from the centre of each cube. In those experiments where two cubes were separated in space, the two fixation beads were made to coincide so that the two cubes were 7 in. apart, centre to centre, symmetrical about the fixation point. Where a single cube was used the same off-centre fixation point was used so that all the results were comparable. The further the cube is out into the visual periphery the less is the satiation period, as space cues become progressively weaker (see Fig. 3). Care was taken therefore to keep the cubes at an equal distance from the fixation point.

In all the transfer experiments described below, except the colour transfer experiment, a plate of glass was inserted at 45° to the line of sight as shown in Figure 2. By alternating the lighting, either cube *A* could be seen through the glass or cube *B* by reflection in it. The subject controlled the lights and could thus switch over from one cube to the other when satiation had occurred.

Ten subjects served in the initial studies but for the transfer experiments two subjects served throughout. Readings were never taken with less than 15 min. interval, so as to avoid after-effects and never more than five per day for any one subject.

To have put more than two subjects through all the experiments would have been too laborious and unnecessary in view of the close agreement and high significance of the results. Anticipation, or suggestion, it is felt were not important factors for before the experiments were performed the outcome was not in any way guessed. All attempts on the part of subjects to influence the first reversal by attitude, failed. When reversal is about to occur a chance blink can bring it on, so that blinking was inhibited as much as possible in the experiments, in any case this was likely to be a constant as between one condition and another.

A few subjects did not always get a clear cut reversal but rather a rapid oscillation of the cube. They were asked to regard this as a reversal. The two principal subjects always got clear cut reversals.

Experiment Series A—Peripheral factors eliminated

The first thing was to ensure that simple peripheral factors were not the cause of the satiation effect.

Experiment A1

One possibility is that the eyes execute saccadic movements when regarding the rotating cube. As these fatigue, movements of opposite sweep may occur and induce an apparent change in direction.

To test this, two cubes were rotated simultaneously in opposite directions, on either side of the fixation point. The apparent reversals tended to occur together. The eyes can not follow two opposing rotations at once and could not therefore be causing both reversals. Pheiffer, Eure and Hamilton (1956) and others have shown, with Necker cubes, that eye movements tend to follow reversals, not precede them.

Experiment A2

Two subjects fixated on the stationary bead for 3 min. The rotating cube was then exposed and the satiation period measured. The means of 10 readings for each subject did not vary significantly from the means obtained without a prefixation period.

It is concluded that fatigue due to the maintenance of fixation is not a factor in the satiation effect.

Experiment A3

Two subjects followed a fixation mark rotating with diameter subtense of 7° . After 3 min. the cube was exposed and the satiation period measured. The means for each subject were again not significantly different from the means obtained without prior eye movements.

It is concluded the fatigue due to any eye movements is not a factor in the satiation effect.

Thus both the intrinsic and extrinsic eye muscles can not be playing a part in the satiation effect, this conclusion is strengthened by the results of experiment C1, where it is shown that there is no spatial transfer of the satiation period. Each part of the retina behaves independently, which would not be the case if the effect were due to eye movement or focussing effects.

Experiment A4

The satiation period may be related to the movement after-effect as shown in the spiral after-effect. There are arguments against this however. In a rotating skeletal cube there is as much movement one way as the other, the nett effect would cancel out. It is well known (Wohlgemuth, 1911) that no movement after-effect results when two opposed striped patterns move over the same field. The following experiment confirms this argument.

Two subjects prefixed a moving striped drum for 3 min. before measuring their satiation. time A similar set of readings was taken with the striped drum seen rotating behind the cube whilst satiation was proceeding. The two motions were opposed in half the trials and in the same direction in the other half.

In no case was the mean satiation time significantly different from the normal.

*Experiment Series B—The satiation process demonstrated**Experiment B1.—The binocular satiation period.*

Ten subjects served. They were asked to look into the black box, the eye-shutters were initially closed, at a timed instant one shutter was opened, giving the subject a monocular view of the cube. The subject was told to fixate the white bead and press a key when the cube reversed its direction of rotation. This is the monocular satiation period. As soon as this occurred, the other eye shutter was opened giving a binocular view of the cube. The cube immediately resumed its true perspective and direction. The subjects were asked to record the next reversal. This is the binocular satiation period. From Table I it is clear that the binocular period is far in excess of the monocular.

TABLE I

MONOCULAR COMPARED WITH BINOCULAR SATIATION PERIODS AND THE UNWINDING EFFECT FOR TEN SUBJECTS. EXPERIMENTS B1 AND B2

	<i>Before objective reversal of cube's direction</i>		<i>After objective reversal of cube's direction</i>
	<i>Monocular S.P.</i>	<i>Binocular S.P.</i>	<i>Binocular S.P.</i>
Means—50 readings	6.2 sec.	15.3 sec.	24.5 sec.
	σ	σ	σ
	2.3	7.3	8.2
Differences very significant			

This is very good evidence that the binocular satiation period is predominantly a central phenomenon occurring above the optic chiasma, its generality is demonstrated by the fact that all the subjects showed the same long binocular satiation period.

Two subjects recorded both the satiation periods and the reversal rate after reversals started.

The results are set out in seconds in Table II.

TABLE II

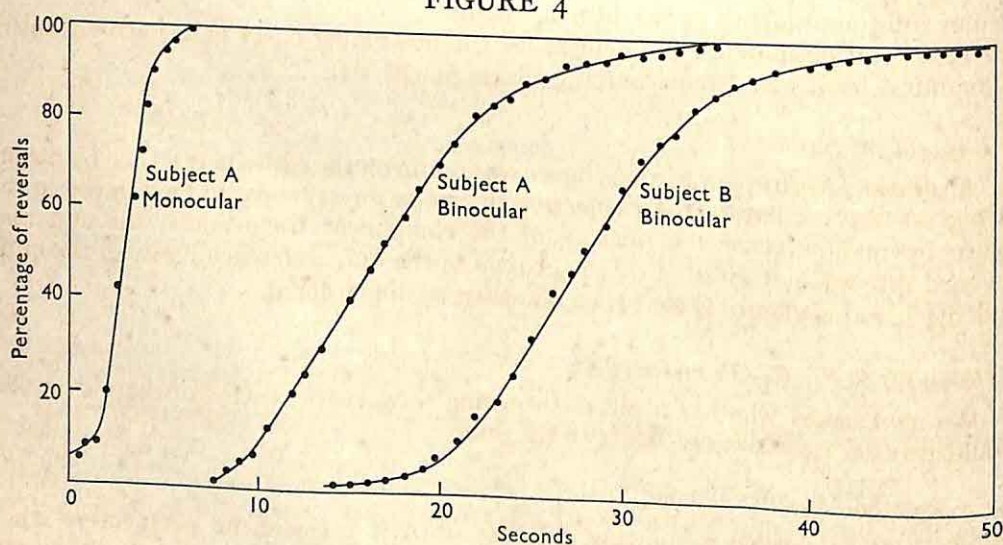
SATIATION PERIODS AND SUBSEQUENT REVERSAL RATES FOR TWO SUBJECTS.
EXPERIMENT B1

	Subject A	Subject B
Mean Binocular S.P.—50 readings	18 sec.	30 sec.
σ	5.5	6.0
Range	8 $\rightarrow$ 36	14 $\rightarrow$ 50
Mean period of Oscillation	3.0 sec.	3.0 sec.
σ	1.2	1.0
Range	1 $\rightarrow$ 8	1 $\rightarrow$ 5

It may be seen from Table II that the individual differences are significant. The binocular satiation period is much longer than the subsequent mean period of reversal, the two ranges do not in fact overlap. The probability of seeing the reversed perspective rises from 0 to 100 per cent. over a period of about 30 sec. The graph in Figure 4 is obtained by plotting the number of reversals in the successive second intervals between the subject first looking at the cube and 50 sec. later, for 100 trials.

Here, therefore, are direct, clear-cut demonstrations of a satiation process. After we had demonstrated these effects, a brief, qualitative description of a similar phenomenon was found in a paper by McDougall written in 1906. He noted that when looking side-on at a rotating toy windmill, the arms reversed when one eye was closed, and reversed to normal as soon as it was opened again.

FIGURE 4

*Experiment B2*

The process of satiation was also demonstrated by a second effect. In this we exposed a rotating cube for a time sufficient to obtain the first apparent reversal of direction. The objective direction of rotation was then changed and the satiation

time measured again. Table I shows that the second satiation time exceeds the first to a significant degree in the 10 subjects tested.

As a control, in some trials, instead of reversing the objective direction of rotation the cube was rapidly reversed twice so that the original direction was restored. Reversal rate was not significantly interrupted, i.e. there was positive transfer of satiation over the interruption. Therefore, the main effect of reversal cannot be due to the momentary interruption of the movement.

It is as if the effect of seeing the cube rotating one way for a while induces a perceptual "winding-up." When the cube is objectively reversed the previous effect has to be wound down, before being wound up sufficiently the other way to allow apparent reversal to occur. This effect may be called the *unwinding effect*. It is a negative transfer of satiation. It means that it is not only the satiation of one perspective alternative which is responsible for the apparent change of rotation in our cube, for if this were so, an objective change of direction when the cube is satiated would leave it still in a satiated, apparently alternating condition. The direction of rotation in depth must play a large part in the satiation process in this type of moving figure. The following two experiments are designed to answer questions about the role of rotation in the satiation effect. We asked whether previewing movement alone or the stationary cube alone would effect the satiation period.

Experiment B3

It has already been shown that moving stripes do not affect the satiation period (Experiment A4).

A stationary cube was prefixed for a period of 3 min. prior to the revolving cube. Again there was no effect on the satiation time.

Then a stationary cube was projected, simultaneously by the half-mirror, into the space occupied by the rotating cube. The satiation time was not affected.

The satiation process involved in the rotating cube must be specific to the particular rotation-in-depth of the stimulus figure.

That rotation-in-depth is essential for the unwinding effect or negative transfer to manifest itself was demonstrated by Experiment B4.

Experiment B4

A rotating quadruple Lissajous figure was set up on the cathode-ray tube by means of two off-beat oscillators. An objective change in direction could be induced in the figure by manipulating the phasing of the component frequencies; the apparent reversal rate was not affected by the change. The only difference between the cube and the Lissajous figure is that the cube is three-dimensional.

Experiment Series C—Transfer effects

We next asked whether a second, rotating solid shape, such as a wire cylinder, would transfer its satiation effects to the cube.

Experiment C1

The subject fixated a rotating cylinder until it reversed its perspective, then switched the lights so that the half-reflecting glass projected a cube into the visual space previously occupied by the cylinder. In alternate trials the cylinder rotated in the opposite direction to the cube. Two subjects did 20 readings each. The satiation period of the cube remained its normal value under both conditions. A control condition using two identical cubes showed very significant transfer, that is

when they were going in the same direction the cube exposed second had a much reduced satiation period; when they were rotating in opposite directions the satiation period of the second cube was prolonged, that is the "unwinding" phenomenon occurred.

In the second part of this experiment the cube and cylinder were projected together in the same space. The cube seemed to be inside and just touching the cylinder. The two were accurately synchronized.

Their satiation periods were similar and they tended to reverse together at first but after prolonged viewing they occasionally rotated in apparently opposed directions, showing that the two processes were not necessarily "locked." Further experiments are planned to study what pattern combinations give the most "lock," this should provide some measure of the strength of configuration tendencies for various pattern combinations.

We concluded from the experiments that a figure showing satiation is highly specific to itself. We next asked whether the satiation effect could transfer to other parts of the visual field.

Experiment C2

Two cubes were placed symmetrically about the fixation bead, 7 in. from centre to centre. Cube A was exposed and allowed to satiate, it was then darkened and cube B illuminated. In half the trials the reverse order was carried out. Two subjects did 20 readings each. The satiation time of the second cube was not affected by the first cube. The satiation process in this case is therefore locus specific as well as form specific.

Experiment C3

This was further demonstrated by asking the subjects to follow a rotating fixation point just behind the rotating cube. We predicted that satiation would be slowed down because we were not providing the visual system with a constant locus of stimulation. This is just what was found, the satiation time was on average four times longer than with steady fixation when the diameter of the circle of movement subtended 7° to the eye. Incidentally this partly explains why the satiation time is longer for a rotating cube than for a static one; with a static figure a particular group of cells is continuously stimulated and therefore soon satiated; with a rotating figure each cell only receives intermittent stimulation. "Cells" here does not refer to retina cells necessarily.

It is concluded from Experiments C1, C2, and C3, that satiation, in this situation at least, is an autoinhibitory process set up by the specific stimulus present and capable of inhibiting the depth-rotation interpretation of that stimulus but of no other pattern or that pattern at any other locus. This supports Köhler's theory.

Experiment Series D—Brightness and colour

Mull, Locke and Ord (1954) and others have argued that a brighter light should cause quicker satiation as measured by mean reversal rate of ambiguous figures. But this is not necessarily so, it would depend on whether the rate of satiation is a constant, an exponential function of brightness or a linear function of brightness. It would also depend on whether the rate is a linear or exponential function of time.

Experiment D1

We found that the satiation time did not change significantly with a change of brightness of four log units. With very dim illumination it fell off, probably because now the figure no longer looked solid.

This independence between the satiation period and brightness suggests that the satiation rate is an exponential function of brightness and time. With high brightness, the initial rate of satiation of the extra bright stimulus is so rapid as not to affect the total satiation time appreciably.

It may be, however, that satiation rate and brightness are truly independent. This would be so if the mechanism of satiation involved here operates at a level in the perceptual mechanism "above" that of the level of brightness discrimination. The fact that cues to depth so radically alter the satiation period supports this interpretation.

Köhler and Wallach (1944) postulated that a reversal of figure-ground illumination should not affect the satiation process in a Rubin cross or the figural-after-effects. Hochberg (1950) found a "directional" effect of this kind in a Rubin cross. We put the matter to test in Experiment D2.

Experiment D2

We asked whether there is transfer of satiation time from a black cube rotating on a white ground to a white cube on a black ground and vice versa. The half reflecting-glass technique was used as before. The results are given for two subjects in Table III. It can be seen that satiation was fully transferred between the two conditions.

TABLE III

TRANSFER OF SATIATION WHEN THE FIGURE-GROUND ILLUMINATION IS CHANGED.
EXPERIMENT D2

	<i>S.P. before change of fig.-ground ill.</i>	<i>S.P. after change of fig.-ground ill.</i>	<i>S.P. after flick of light in control condition</i>
Subject A—10 readings . .	15.4 sec.	6.6 sec. $\sigma = 2.7$	6.5 sec. $\sigma = 2.2$
		Difference not significant	
Subject B	22.0 sec.	4.7 sec. $\sigma = 1.0$	6.6 sec. $\sigma = 2.3$
		Difference not significant beyond 0.1 level	

It is difficult to draw definite conclusions from this experiment for the situation is not equivalent to the one which Köhler and Hochberg used, but so far as they go, they support Köhler.

Finally an experiment was conducted to decide whether there is transfer of satiation time from a green to a red cube and vice versa.

Experiment D3

Wratten filters Nos. 25 and 58 were used. A cube was illuminated by one and then the other colour in alternating order on each trial. The results are given in Table IV. The control condition as in all these experiments consists in switching over from one white cube to another identical cube.

TABLE IV
TRANSFER OF SATIATION FROM ONE COLOUR TO ANOTHER.
EXPERIMENT D₃

	S.P. for 1st colour	S.P. for 2nd colour	S.P. after flick of light in control condition
Subject A—32 readings . .	20 sec.	8.5 sec. $\sigma = 5.2$	4.7 sec. $\sigma = 3.6$
		Difference significant at 0.05 level	
Subject B—32 readings . .	27 sec.	5.5 sec. $\sigma = 3.1$	2.3 sec. $\sigma = 1.4$
		Difference significant at 0.01 level	
Mean S.P. for green . .	Subject A 20 sec. Subject B 29 sec.	Differences not significant	
Mean S.P. for red . .	Subject A 21 sec. Subject B 25 sec.		

Colour seems to be having some small effect here, there is not the same degree of transfer as in the white to white condition. This could be because different receptors are being stimulated by the different wavelengths. The other possibility is that there are more cues to depth present with one colour than the other, perhaps due to differing amounts of chromatic aberration. This would show as a longer satiation period for one of the colours, but as the figures show, this is not the case. The relative absence of transfer with colours is therefore another aspect of the absence of spatial transfer of satiation.

DISCUSSION

The problem of three-dimensionality and satiation

Köhler (1939) used a flat, static Rubin Cross to show that reversal rate increases with duration of fixation. Here it is possible to think, as Köhler did, in terms of a spatial distribution of figural processes in the cortex. This does not fit the case of a solid reversible cube, for here there is no shift of figure location except in the third dimension. Köhler holds that the isomorphic cortical field is three-dimensional but it has always been difficult to reconcile this and Köhler's insistence that the cortex acts as a volume, electrolytic conductor, with the known properties of the cortex. The cortex is virtually two-dimensional. Lashley, Chow and Semmes (1951) short-circuited the surface of the cortex with metal strips, Sperry, Miner and Meyers (1955) isolated parts of the cortex laterally by implanting non-conducting strips and Thomas and Stewart (1957) used D.C. stimulation of the cortex. None of these procedures affected pattern discrimination ability in the animals concerned.

If the electrolytic, non-neuronal theory of cortical integration were discarded, it would still be possible to retain Köhler's theory of satiation as stated earlier in this paper but it would have a neuronal basis.

As an alternative to Köhler's simple tridimensional isomorphism, it may be that depth cues are decoded and integrated in a cortical centre which deals with this type of information abstracted or "filtered" from other informational content, such as

spatial location, colour, etc. However, the absence of transfer in the satiation of the perspective of a three-dimensional cube suggests that the three-dimensional and movement aspects of a figure are not "abstracted" from "location" in cortical processes, at least prior to the symbolic-conceptual level of functioning. That is not to say that there are not different "centres" responsible for decoding and integrating location, movement, depth, etc., but that "position" or "local sign" is carried through these several processes in the perceptual mechanism.

There is evidence for assuming a separate centre for three-dimensionality. Critchley (1953, p. 300) quotes a case of Riddoch's who had a bullet wound in the left parietal and right occipital areas. Everything he saw was flat, landscape was like a piece of stage scenery. The case was possibly complicated by some agnosia, so that any conclusions must be tentative.

The problem of a moving stimulus and satiation

Then there is the problem of the movement of the cube. As Smith (1948) pointed out, it is difficult to see how Köhler's theory or the statistical theory of Osgood and Heyer (1952) can account for after-effects produced by moving stimuli. On these theories a homogeneous area or, in the case of three-dimensional movement, a volume, of satiation should result and the identity of the moving pattern should be "washed out."

The evidence is not unanimous that satiation can occur without fixation. In the movement-after-effect, an after-effect is produced which is related to the direction of movement of the inspection lines, whether or not the subject fixates the lines. Gibson (1933) got his effects, of viewing the world through prisms, without fixation. Carlson (1959) used moving, curved lines and still got evidence of a figural after-effect. On the other hand, Summerfield and Miller (1955) found that fixation was necessary to produce an after-effect of viewing the radiating line pattern of the Herring-bone illusion.

Our cube clearly satiates in spite of its movement and this does not transfer to other shapes, therefore, the rotation of the cube does not result in a homogeneous area or volume of satiation in the cortex but a specific movement-in-depth-cubic-pattern of satiation. This finding contradicts Köhler's theory of non-neuronal conduction.

George (1953), Deutsch (1955, 1956) and Spitz (1958) have each tried to reconcile one or other theory of the figural-after-effects with a movement of the inspection figure, especially the movement-after-effect. What they all lack and what Köhler's volume-conductor theory lacks, is a mechanism which is direction sensitive to a series of moving repetitive stimuli and which can maintain the object constancy or figural identity of the moving shape.

As an example, take Deutsch's theory. He postulated that contours radiate impulses, in an array of receptor cells, at right-angles to the length of the contour but that this propagated wave will be impeded if it passes into a previously excited area. Thus when a line moves across the retina the wave propagated behind the line will be impeded as it flows into cells which have just been stimulated by the line, but will not be impeded in front of the line. This differential rate of flow is the basis of the directional sensitivity of the visual mechanism and accounts for the movement after-effect, for when the stimulus is removed, there occurs rebound. The theory breaks down when several equally spaced moving stimulus lines are considered, for now both the forward and the backward propagated waves flow into areas which have just been stimulated. The net effect would cancel out. A similar argument applies to the other theories mentioned.

Bowditch and Hall (1881) postulated a cortical movement centre for the appreciation of movement. Exner (1888) suggested that the eye-movement centres decode both the velocity and direction of moving stimuli. Something is known of how movements of the retinal-image which result from eye or head movements are compensated and it is known that eye movement centres are involved. It is not clear how such relatively simple compensatory mechanisms could operate when separate parts of the visual world move in opposed directions, as in the rotating cube or spiral after-effect. A highly elaborate system must be assumed which can achieve object constancy in spite of complex movements and this requires a high degree of integration of information, retaining its specific content, from wide areas of the visual field. Perhaps movement is, as it were, "taken out" of the information flow as a matrix of successive differentials. In this way patterns of movement would become coded as, specific, fixed spatial patterns of activity. Satiation would then proceed in this fixed pattern. Otherwise one would have to assume that it is a particular sequence of spatially distributed nerve impulses which is satiated. Nothing can, as yet, be said to decide between these possibilities, but whichever is the case, Köhler's theory of electrolytic conduction would fail to be supported, as would other theories, such as those mentioned, which are based on the idea of a general non-specific integrating mechanism. For, on the one hand, they could not account for integration between separate centres and on the other could not account for the satiation of specific sequences of impulses. The evidence in this paper shows that satiation proceeds in specific sequences or their coded spatial counterparts and thus reveals the essential weakness of these non-specific theories.

In conclusion, Köhler's theory of satiation is supported in a formal way, but not his theory of non-neuronal conduction. Perhaps auto-inhibition is a better term than satiation. Further experiments are in progress in which auto-inhibitory processes are being studied to answer the following questions.

- (1) What is responsible for the individual differences which the present study shows are significant and consistent from day to day? Possibilities are: stereoscopic acuity; fixation steadiness; the cortical-inhibitory processes; metabolism, etc.; age.
- (2) How long will the satiation processes persist after the removal of the stimulus?
- (3) Do other situations of biased ambiguity behave in a similar manner, with regard to the effect of the amount of bias on satiation time; the presence of spatial and pattern transfer, etc? Other biased ambiguity situations which we hope to look at include; the stereokinetic phenomena of Wallach and O'Connell (1953); the ambiguous movement patterns of Johansson (1950); puzzle pictures. There is no *a priori* reason for supposing that auto-inhibitory processes always behave in the same way.

REFERENCES

- ADAMS, P. A. (1954). The effect of past experience on the perspective reversal of a tridimensional figure. *Amer. J. Psychol.*, **67**, 708-10.
- BOWDITCH, H. P., and HALL, G. S. (1881). Optical illusions of motion. *J. Physiol.*, **3**, 296-301.
- BROWN, K. T. (1955). Rate of apparent change in a dynamic ambiguous figure as a function of observation time. *Amer. J. Psychol.*, **68**, 358-71.
- CARLSON, V. R. (1953). Satiation in a reversible perspective figure. *J. exp. Psychol.*, **45**, 442-8.
- CARLSON, V. R. (1959). After-effect of a moving pattern. *J. exp. Psychol.*, **58**, 31-9.
- COHEN, L. (1959). Rate of apparent change of a Necker cube as a function of prior stimulation. *Amer. J. Psychol.*, **72**, 327-44.

- CRITCHLEY, M. (1953). *The Parietal Lobes*. London.
- DAY, R. H. (1958). On interocular transfer and the central origin of visual after-effects. *Amer. J. Psychol.*, **71**, 784-90.
- DEUTSCH, J. A. (1955). A theory of shape recognition. *Brit. J. Psychol.*, **46**, 30-7.
- DEUTSCH, J. A. (1956). The statistical theory of figural after-effects and acuity. *Brit. J. Psychol.*, **47**, 208-15.
- DONAHUE, W. T., and GRIFFITHS, C. H. (1931). The influence of complexity on the fluctuations of the illusions of reversible perspective. *Amer. J. Psychol.*, **43**, 613-7.
- DUKE-ELDER, W. S. (1932). *Textbook of Ophthalmology*. London.
- EXNER, S. (1888). Über optische Bewegungsempfindungen. *Biologisches Centralblatt*, Bd. 8, No. 14.
- FISCHELLE, V. R. (1957). Reversible perspective in Lissajous' figures: some theoretical considerations. *Amer. J. Psychol.*, **60**, 240-9.
- FLUGEL, J. C. (1913). Influence of attention in illusions of reversible perspective. *Brit. J. Psychol.*, **5**, 357.
- GEORGE, F. H. (1953). On the theory of the figural after-effects. *Canad. J. Psychol.*, **7**, 167-71.
- GIBSON, J. J. (1933). Adaptation, after-effects and contrast in the perception of curved lines. *J. exp. Psychol.*, **16**, 1-31.
- HOCHBERG, J. E. (1950). Figure-ground reversal as a function of visual satiation. *J. exp. Psychol.*, **43**, 682-8.
- JOHANSSON, G. (1950). *Configuration Event Perception: an experimental study*. Uppsala.
- KÖHLER, W. (1939). *Dynamics in Psychology*. London.
- KÖHLER, W., and WALLACH, H. (1944). Figural after-effects. An investigation of visual processes. *Proc. Amer. Philos. Soc.*, **88**, 269-357.
- LANGDON, J. (1951). The perception of a changing shape. *Quart. J. exp. Psychol.*, **3**, 157-65.
- LASHLEY, K. S., CHOW, K. L., and SEMMES, J. (1951). An examination of the electrical field theory of cerebral integration. *Psychol. Rev.*, **58**, 123-36.
- MCDUGALL, W. (1906). The physiological factors in the attention process. *Mind*, **15**, 329-59.
- MC EWEN, P. (1958). Figural after-effects. *Brit. J. Psychol.*, Monograph Suppl. No. 31.
- MULL, H. K., LOCKE, N. M., and ORD, N. (1954). The effect of two brightness factors upon the rate of fluctuation of reversible perspectives. *Amer. J. Psychol.*, **67**, 341-2.
- OSGOOD, E. E., and HEYER, A. W. (1952). A new interpretation of figural after-effects. *Psychol. Rev.*, **59**, 98-118.
- PHEIFFER, C. H., EURE, S., and HAMILTON, C. B. (1956). Reversible figures and eye-movements. *Amer. J. Psychol.*, **69**, 452-5.
- SMITH, K. R. (1948). The satiation theory of the figural after-effects. *Amer. J. Psychol.*, **6**, 282-6.
- SPERRY, R. W., MINER, N., and MEYERS, R. W. (1955). Visual pattern perception following subpial slicing and tantalum implantations in the visual cortex. *J. comp. physiol. Psychol.*, **48**, 50-8.
- SPITZ, H. H. (1958). Neural satiation in the spiral after-effect and similar movement after-effects. *Percept. mot. Skills*, **8**, 207-13.
- SUMMERFIELD, A., and MILLER, K. M. (1955). Visual illusion and figural after-effects with and without fixation. *Quart. J. exp. Psychol.*, **7**, 149-58.
- THOMAS, G. J., and STEWART, P. A. (1957). The effect on visual perception of stimulating the brain with polarizing currents. *Amer. J. Psychol.*, **70**, 528-40.
- WALLACH, H., and O'CONNELL, D. D. (1953). The kinetic depth effect. *J. exp. Psychol.*, **45**, 205-17.
- WOHLGEMUTH, A. (1911). On the after-effect of seen movement. *Brit. J. Psychol.*, Monograph Suppl., No. 1.

THE EFFECT OF TWO "TRANQUILLIZERS" ON AUDITORY DISCRIMINATION AND DELAYED RESPONSE PERFORMANCE OF MONKEYS

BY

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Five rhesus monkeys were tested on performance of an auditory discrimination and a delayed response test with a sliding scale of increasing difficulty after treatment with (a) 0.2 to 0.5 mgm./kgm. reserpine, (b) 150 mgm./kgm. meprobamate, (c) an equivalent saline injection, and (d) no treatment. Both drug treatments resulted in a severe deficit on the auditory discrimination and no change or improvement in delayed response performance. The auditory deficit was not due simply to an increased latency of response.

It is suggested that these results support the hypothesis that reserpine and meprobamate reduce the utilization of sensory information.

INTRODUCTION

There is a rapidly-growing experimental literature on the effects of "tranquillizing drugs" on human and animal behaviour. Most investigators have concentrated on the "conditioned emotional" sphere of behaviour, while there has been a paucity of work on the effects of these drugs in such areas as discrimination and motivation. One suggested *general* characterization of the behavioural effects of tranquillizer action has been that these agents exert their effect on behaviour by reducing sensory input (Weiskrantz, 1957). The present communication reports a test of a specific formulation of this hypothesis, viz. are reserpine and meprobamate, two tranquillizers differing over a wide range of behavioural and physiological properties (Berger, 1957), able to decrease the utilization of information by a rhesus monkey in performing an auditory discrimination? In order to study the specificity of the drug effect, the animals were also examined on another type of test, spatial delayed response.

METHOD

Subjects

Five untamed rhesus monkeys (*Macaca mulatta*) ranging in weight from 3.6 to 4.5 kgm. were used as subjects. Throughout the experiment they were maintained on an *ad lib.* diet of lab. chow and water. They had received neither drug previously.

Apparatus

Both tests utilized a Wisconsin General Test Apparatus (WGTA). The experimental room was darkened, the only illumination being a 60-watt frosted bulb in the top of the WGTA.

Test procedure

Auditory discrimination

The monkeys were trained to perform a discrimination between white noise and a 1,000 c.p.s. tone in a "go—no-go" (successive) discrimination situation. The white noise was 68 dB. and the tone 67.5 dB. as measured by a Dower Sound Level Meter Type 1400 C. The speaker for both sound sources was located behind the experimenter, about 4 ft. from the monkey and at the level of his head. Five sec. after the sound stimulus was turned on the opaque screen of the WGTA was raised for a 5-sec. period during which the monkey faced a single cup covering a food well. If it displaced the cup within this 5-sec.

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period when the white noise (positive stimulus) was on, it found a small piece of apple in the food well. Conversely, if it refrained from responding in this period when the tone (negative stimulus) was on, it received a piece of apple by hand. However, if it either displaced the cup when the negative stimulus was on or did not displace the cup when the positive stimulus was on, it did not receive any apple during that trial. The stimulus was turned off and the opaque screen dropped after the cup was displaced or at the end of the 5-sec. period, whichever came first. The sequence of stimuli was determined from a random table so adjusted that the number of positive stimuli presented equalled the negative ones in each session.

Spatial delayed response.

Two identical cups covered two food wells located 7 in. apart. They were separated by a thin vertical partition to prevent the monkey from displacing both cups in a single trial by moving its hand quickly from one cup to the other. At the start of each trial the cups covered both food wells. When the opaque screen was raised the experimenter showed the monkey a piece of apple for 5 sec., displaced one cup and placed the food in the well under it, replaced the cup and then lowered the opaque screen for a specified delay interval. The delay interval was determined by a sliding scale of increasing difficulty. If the animal did not make an error in four consecutive trials the delay interval was increased one step, from an initial interval of 5 sec. to a maximum of 140 sec. The steps were 5, 7, 15, 20, 25, 30, 40, 60, 100 and 140 sec. The sequence of cups baited was determined from a random number table so adjusted that both cups were baited within each step at least once and both cups were baited an equal number of trials within each session.

No correction trials were given in either problem.

Pre-drug training schedule

Previous to this experiment none of the animals had had any experience in a WGTA. Their only test experience had been in a Skinner Box, where they were trained on fixed ratio and differential reinforcement of low rates schedules with blackcurrant juice as the reinforcement (Gross, Oxbury and Weiskrantz, 1960).

After they learned to displace the cups for pieces of food they were trained successively to a criterion of 90 correct in 100 trials on 1 sec., 5 sec., and 10 sec. spatial delayed response tests and then on the auditory discrimination. Training consisted of 40 trials per day.

Dosages and schedule of testing

Meprobamate. The meprobamate dosage was 150 mgm./kgm. injected intramuscularly in a vehicle of polyethylene glycol 400 (Wallace). The animals were tested alternately either 1 hr. after injection with meprobamate or as controls. Test sessions occurred every other day. All five animals received at least five sessions of drug treatment and five intervening control sessions. Each session consisted of 60 trials sliding-scale delayed response and 30 trials auditory discrimination. Control procedures were either an equivalent volume injection of physiological saline or no treatment. The interval between injection and testing was equal for drug and saline control sessions.

Reserpine. Reserpine was injected intramuscularly in a distilled water vehicle (Ciba). The dose levels and injection-testing interval are indicated in Table II. Four monkeys were used as subjects. Animals 14 and 16 received 60 trials sliding-scale delayed response and 30 trials auditory discrimination per experimental session. Animals 12 and 17 received 40 and 20 trials per session respectively on the two tests. At least one week intervened between successive reserpine injections. Control sessions were given 1 or 2 days before each drug injection. Control procedures were the same as those for meprobamate. Since animals tend to become non-reactive under heavy doses of reserpine, the dosage was varied in an attempt to obtain information on the maximal effect of the drug that is possible without contamination by the animal's refusal to respond. Time of testing following injection was varied for the same reasons. For example, with a heavy dose (0.5 mgm./kgm.), it was necessary to wait 20 hr. after injection before the animal was sufficiently responsive (see Table II).

Treatment of data

With some doses of the drugs on some occasions, animals did not complete all of the trials in a session. When this occurred, the entire session (both the auditory and delay problems) was rejected except when it could be matched with the previous control session in which approximately the same number of trials on each problem had been performed

by the animal. These adjustments are indicated in the footnote to Tables I and II. The object of this treatment was to compare control and experimental sessions without contamination by periods in which the animals were not responding.

A one-tailed *t*-test was used for the auditory data and a two-tailed one for the delay data in accordance with the hypothesis being tested.

RESULTS

General observations

Meprobamate. The experimental dosage of 150 mgm./kgm. occasionally caused slight incoordination. However, we never observed anything that could be construed as a "taming effect" at this or higher dosages as has been reported (Berger, 1954, 1957). High doses (e.g. 400 mgm./kgm.) caused partial or complete "paralysis." In this condition the monkeys appeared to be even more frightened of humans than normally, presumably because of their inability to move away.

Reserpine. Doses of reserpine equal to or larger than the highest experimental dosage (0.5 mgm./kgm.) caused what appeared to be an extremely rapid habituation or long "refractory" period following any stimulus. This "non-reactivity" is currently under study.

Auditory and delayed response test

On the auditory task, the animals made a total of 56 errors in 580 trials after injection of an equivalent volume of saline and 33 errors in 360 trials after no treatment. On the delay task they made a total of 266 errors in 1,116 trials after injection of an equivalent volume of saline and 184 errors in 720 trials after no treatment. The differences between performance after saline injection and after no treatment did not approach significance. Consequently both groups of control data were pooled.

After treatment with either meprobamate or reserpine at the stated dosages, all animals showed a severe deficit on performance of the auditory discrimination (Tables I and II). No animal showed a significant deficit on delayed response performance. In fact, four of the five animals slightly improved their delayed response after meprobamate treatment (only one of them significantly). Similarly, three of the four animals treated with reserpine slightly improved their delayed response performance after treatment with this drug.

Essentially the same picture occurred using the average maximum delay period reached by each subject under each condition. (These values were computed by transforming the delay values into their ordinal ranks before averaging.)

DISCUSSION

The lack of significant change in delayed response performance following reserpine treatment when the data for all the monkeys are grouped agrees with the findings of Hall, Warren and Harlow (1955) and Riopelle and Pfeiffer (1958). Perhaps the reasons that our animals tended to improve slightly in the delayed response after drug treatment were that we used a sliding scale of difficulty (neither Riopelle and Pfeiffer, nor Hall *et al.*, used intermediate delay periods; their animals performed either near chance or over 85 per cent.) and our animals were less extensively trained.

Since sensory discrimination is necessarily involved in delayed response performance, how can the hypothesized deficit in the utilization of sensory information result in a lack of decrement or even an improvement in such performance? Malmö's work (1942) suggests that factors tending to reduce sensory stimulation in the delay period lead to an improvement in delayed response, at least in monkeys with frontal cortical lesions. Riopelle and Pfeiffer (1958) have shown that reserpine can improve delayed response of brain-operated animals, and suggest similarly, that their result

TABLE I
EFFECT OF MEPROBAMATE ON PERFORMANCE

Monkey	Auditory discrimination				Delayed response			
	Meprobamate		Control		Meprobamate		Control	
	Trials	Errors	Per cent. correct	Trials	Errors	Per cent. correct	Max. delay (sec.)	Trials
12	180	86***	52	150	16	89	17	300
13	150	49***	67	210	14	93	17	420
14	150	34*	77	150	9	94	25	300
16	180	68*	62	90 ¹	9	90	60	180 ¹
17	210	63*	70	150	22	85	25	300
Total	870	300***	66	750	70	93	—	1500

* = $p < 0.05$.** = $p < 0.01$.*** = $p < 0.001$.

¹ Two delayed response control sessions were not completed and hence are not included. Subject stopped responding after 20 and 17 trials. Similarly the data for auditory performance in the same sessions are not included. Subject made 13.3 per cent. errors in one of these auditory sessions and 10 per cent. in the other.

TABLE II
EFFECT OF RESERPINE ON PERFORMANCE

Monkey	Auditory discrimination				Delayed response			
	Reserpine		Control		Reserpine		Control	
	Dosage (mgm./kgm.)	Hours after injection	Trials	Errors	Per cent. correct	Trials	Errors	Per cent. correct
12	0.30	2	20	7	65	40	14	65
14	0.25	2	60	15	75	101 ¹	25	85
16	0.50	20	30	13	57	60	24	80
17	0.40	2	20	10	50	15 ²	4	73
Total			130	45**	65	210	67	80

** = $p < 0.01$.¹ Only 41 trials of drug and following control session were completed.² Only 15 trials of drug and following control session were completed.

may have been due to decreased distractibility. Obviously, however, reserpine under these conditions cannot lead to *complete* sensory blockade, or the animals would not discriminate the baiting of the cup. The suggestion made here is that stimulus detection may still occur under certain very favourable circumstances—with intense stimuli or with strongly reinforcing stimuli, such as a piece of apple, presented in such a way as to maximize attention as is typically done during a delayed-response baiting session. Without such optimal conditions, however, detection may be damped. A functionally similar description would be one which stated that the background noise level is higher, so that only the stimuli least likely to be masked out by the noise would be detected.

Riopelle and Pfeiffer (1958) have reported that reserpine had no effect on the learning of a visual multiple discrimination task by monkeys. This possible discrepancy between our results and theirs may be related to one or more of the following considerations: (a) Reserpine may decrease the inflow of auditory information and not affect the information necessary for visual problems. However, Wilson (Weiskrantz, 1957) found "visual discrimination performance begins to deteriorate as a function of dosage and time after reserpine injection, first becoming random and finally stopping all together." (b) Riopelle and Pfeiffer report that their monkeys "were given extensive training on the test before participating in this experiment and their performance levels had reached stable values." The degree of test sophistication sufficiently developed to enable them to perform at a mean level of 85 per cent. on the second trial of each new problem, which published data indicate would require a large number of trials (Harlow, 1949). Our monkeys had no relevant test experience whatsoever before they were taught the auditory and delayed response problems. Since overtraining has been shown to obscure the effect of a cortical lesion on visual discrimination (Orbach and Fantz, 1958) it is possible that their "extensive" overtraining may have obscured the effects of the reserpine treatment. (c) Perhaps the most important difference between our study and theirs was that their measure, performance on trial two in a multiple discrimination problem, was essentially, as they point out, a "learning measure," whereas our measure was of performance after a criterion had been reached.

The fact that delayed response performance was not impaired after treatment with either tranquillizing agent at dosages which resulted in an auditory discrimination loss suggests that the discrimination loss was not due to a general deterioration of behaviour.

However, it might be argued that the auditory errors after drug treatment were simply due to not responding or to responding less quickly. If one of the drug's effects was to increase the latency of response, as has been shown to be the case under certain conditions (Riopelle and Pfeiffer, 1958; Kornetsky, 1958) then the delayed response performance probably would not be affected since the animal was given a long time to respond (30 sec.). On the other hand, such an increase in latency of response would cause a marked deficit in auditory discrimination performance since in this problem the animal must respond within 5 sec. on half the trials. However, an examination of the types of errors made on auditory discrimination performance after drug injection and on control days (Table III) indicates that most of the errors were due to the animals responding when they should not, rather than to not responding when they should, thus ruling out increased latency of response as the explanation of the results.

Another possible explanation of our results is that the auditory problem was in some way "more difficult" or was characterized by "less stable performance" than the

TABLE III
 ERRORS MADE ON NO-GO TRIALS (PER CENT. OF TOTAL ERRORS)

Monkey						Drug	Control
A. Meprobamate:							
	12	..	..	..	..	89.5	56.3
	13	..	..	..	..	77.6	50.0
	14	..	..	..	..	85.3	77.8
	16	..	..	..	..	88.2	66.6
	17	..	..	..	..	76.2	40.9
Meprobamate mean						75.4	54.3
B. Reserpine:							
	12	..	..	..	..	100.0	0.0
	14	..	..	..	..	100.0	88.9
	16	..	..	..	..	100.0	100.0
	17	..	..	..	..	30.0	66.7
Reserpine mean ..						84.4	63.9

delayed response problem. If this were true then any non-specific interference with the animals' behaviour might be expected to have a greater effect on performance of the auditory discrimination. However, an examination of Tables I and II shows that control performance on the auditory discrimination remained at an average level of 93 per cent. correct, whereas control performance on the delayed response resulted in an average of only 77 per cent. correct trials.

Therefore the original hypothesis, that meprobamate and reserpine decrease the utilization of the sensory information necessary to perform an auditory discrimination in rhesus monkeys, is considered to have been strongly supported by the present experiment.

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REFERENCES

- BERGER, F. M. (1954). The pharmacological properties of 2-methyl-2-n-propyl-1, 3-propanediol dicarbamate (Miltown), a new interneuronal blocking agent. *J. Pharmacol.*, **112**, 413-23.
- BERGER, F. M. (1957). The chemistry and mode of action of tranquillizing drugs. *Ann. N.Y. Acad. Sci.*, **67**, 685-99.
- GROSS, G. C., OXBURY, J. M., and WEISKRANTZ, L. (1960). The effect of meprobamate on auditory discrimination, delayed response and time-interval estimation in Rhesus monkeys. (Abstract.) In BRADLEY, P. B., DENIKER, P. and RADOUCO-THOMAS (Ed.) *Neuro-Psychopharmacol.* Amsterdam.
- HALL, J. F., WARREN, J. M., and HARLOW, H. F. (1955). The effects of reserpine (Serpasil) on the delayed response in monkeys. *J. Psychol.*, **40**, 159-61.
- HARLOW, H. F. (1949). The formation of learning sets. *Psychol. Rev.*, **56**, 51-65.
- KORNETSKY, C. (1958). Effects of meprobamate, phenobarbital and dextro-amphetamine on reaction time and learning in man. *J. Pharmacol.*, **123**, 216-9.
- MALMO, R. B. (1942). Interference factors in delayed response in monkeys after removal of frontal lobes. *J. Neurophysiol.*, **5**, 295-308.
- ORBACH, J., and FANTZ, R. L. (1958). Differential effects of temporal neocortical resections on overtrained and non-overtrained visual habits on monkeys. *J. comp. physiol. Psychol.*, **51**, 126-9.
- RIOPPELLE, A. J., and PFEIFFER, C. C. (1958). Effects of acute and chronic administration of reserpine on test performance. *Arch. Neurol. Psychiat.*, Chicago, **79**, 352-8.
- WEISKRANTZ, L. (1957). Reserpine and behavioural non-reactivity. In GARATTINI, S. and GHETTI, V. (Ed.). *Psychotropic Drugs*. Amsterdam.

VISUAL ORIENTATION PREFERENCES IN THE RAT

BY

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An experiment was performed which demonstrated a preference in hooded rats for vertical rather than horizontal striations. It was demonstrated that experience in discriminating between horizontal and vertical striations facilitates subsequent discrimination between squares and circles, regardless of which type of striation was positive in initial training. This finding has implications for theories of shape recognition, which are discussed.

INTRODUCTION

It has been reported elsewhere (Dodwell, 1957) that rats trained to jump to horizontal striations and to avoid vertical striations subsequently transfer to squares, or other shapes with horizontal base lines; this transfer may be perfect, if the length of the base line of the transfer shape is about the same as that of the striations used in training. This fact was principally responsible for the writer's questioning of Deutsch's theory of shape recognition (Deutsch, 1955) and his suggestion of an alternative theory (Dodwell, 1957). In replying to Dodwell's criticisms, Deutsch suggests that preference for jumping to horizontal lines in the rat is a special case, since in the natural state jumping towards horizontal objects would, generally, have biological utility (Deutsch, 1958). He quotes Lashley's finding (Lashley, 1938) that his rats tended to prefer horizontal to vertical striations in support of this argument. Dodwell then pointed out that this evidence is hardly conclusive, since Lashley's rats had had experience of jumping to open doors before they were trained on horizontal and vertical striations, and jumping to open doors could have involved discrimination of the horizontal base of the open door (Dodwell, 1958 *a*). Hence, the preference might have been learned, rather than "built in."

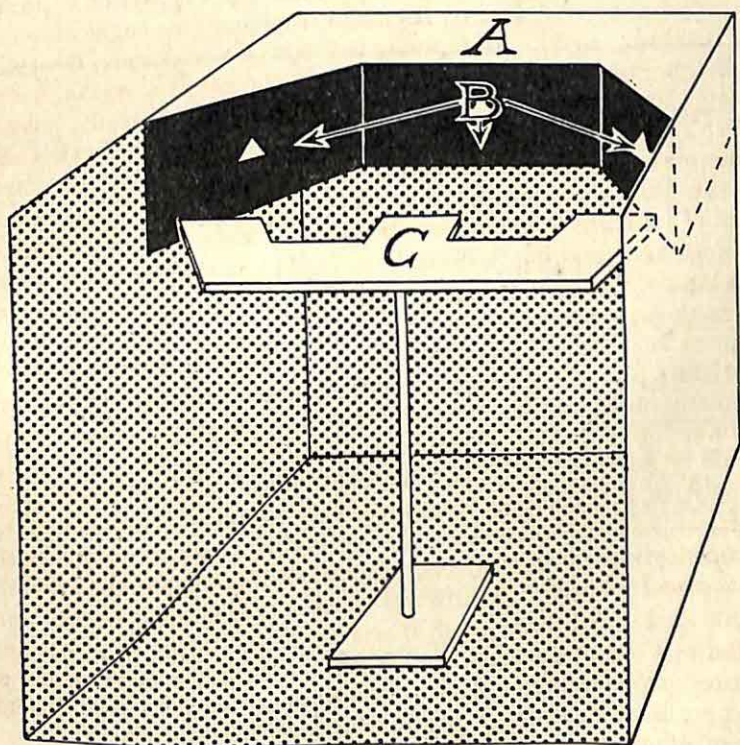
This argument is unsatisfactory; the only reasonable thing to do is to find out whether in fact rats prefer horizontal to vertical striations, when they have had no pre-training on stimuli containing cues of shape or orientation. This was done, and the results are reported below. Deutsch's argument implies (a) that horizontal striations should be preferred initially to vertical striations, (b) that learning to jump to horizontal positive, vertical negative, should be accomplished quicker than learning to jump to vertical positive, horizontal negative, and (c) that more transfer to squares should be obtained when training has been on horizontal positive, than when training has been on vertical positive. Deutsch's theory, incidentally, would not predict transfer in either case. All three implications are tested in the experiment, and the last gives an indirect test of the validity of Deutsch's theory, since his argument about the special nature of preferences for horizontal lines cannot hold for vertical lines as well; if it turns out that training in vertical striations (positive) facilitates the discrimination of square from circle, this evidence will be evidence against Deutsch's theory.

It was decided to investigate preferences for horizontal and vertical quite thoroughly since, despite what has been said, the writer was inclined to think that Deutsch's criticism might not be entirely vacuous.

EXPERIMENT

Apparatus and method. Jumping stands were used; one two-card, and one three-card stand. The two-card stand embodied modifications of Lashley's stand described elsewhere in detail (Dodwell, 1957); namely, the jumping platform was Y-shaped, and the stimulus cards were so arranged that the rat's orientation was normal to the card when it stood on one of the arms of the platform; the cards themselves were 12 in. square, the shape being presented at the centre of this square against a homogeneous black background. These features were also incorporated in the three card stand (see Figure 1). The reason for using two- and three-card stands was the following: it has been claimed (e.g. by Fields, 1953) that multiple discrimination stands lead to more efficient learning than two-card stands. The writer thought it worth finding out whether the three-card stand would give better results than the two-card stand. The differences observed are reported below. A slight complication in using the three-card stand is that rats might learn to discriminate "one different" if always shown one positive with two negative cards, so it is necessary to show one positive and two negative shapes on some trials, two positive and one negative on others. If the two conditions are used with equal frequency, and arranged in a random sequence, the probability of correct choices by chance is the same as for the two-card stand, and the results are directly comparable for the two stands. This arrangement was used for the three-card stand groups in the present experiment. This eliminates the factor of "oddity" learning, which probably is the basis for the greater efficiency of multiple-card stands noted by Fields. A design involving the possibility of oddity learning would invalidate the types of transfer test and training described below, and would therefore limit the usefulness of multiple-card stands for investigations of genuine discriminatory transfer.

FIGURE 1



The three-card jumping stand.

- A, goal-box.
- B, stimulus cards.
- C, jumping platform.

The shapes used for training were horizontal and vertical striations, consisting of $3\frac{1}{2}$ -in. square cards, each with four white and three black $\frac{1}{2}$ -in. stripes. The same cards could be used as either horizontal or vertical striations, since the shapes could be rotated, as described previously (Dodwell, 1957). Transfer shapes—squares and circles—consisted of 3-in. squares and circles of the same area outlined in white ink on matt-black cards. Three different widths of outline were used for each shape, so that the possibility of transfer discrimination in terms of brightness could be controlled (Dodwell, 1957). All animals were trained in the jumping habit on a brightness discrimination, containing no cues of a shapelike character, and no orientational cues, as described elsewhere (Dodwell, 1960). The animals were placed on a water deprivation schedule some days before the beginning of training, having access to water for 2 hr. each day. During the experiment they were run after approximately 20 hr. water deprivation, and could drink in the goal-box. The subjects were 12 male hooded rats (Long-Evans strain) approximately 3 months old at the beginning of the experiment.

Design and procedure. The 12 rats were divided into two equal groups at the end of brightness pre-training; both groups were trained on the discrimination of horizontal from vertical striations with 10 jumps per rat per day, group A having vertical positive, group B horizontal positive. Each group was further divided into two sub-groups, groups AI and BI being trained on the three-card stand, groups AII and BII on the two-card stand. These groups are shown in Table I, together with the numbers in each group. (Unfortunately, an epidemic of some unidentified disease spread through the rat colony during training, during which four of the original 16 animals in the experiment died; hence the unequal numbers in the sub-groups.)

TABLE I
DIVISION INTO FOUR SUB-GROUPS, ACCORDING TO TRAINING PATTERNS
AND TYPE OF STAND

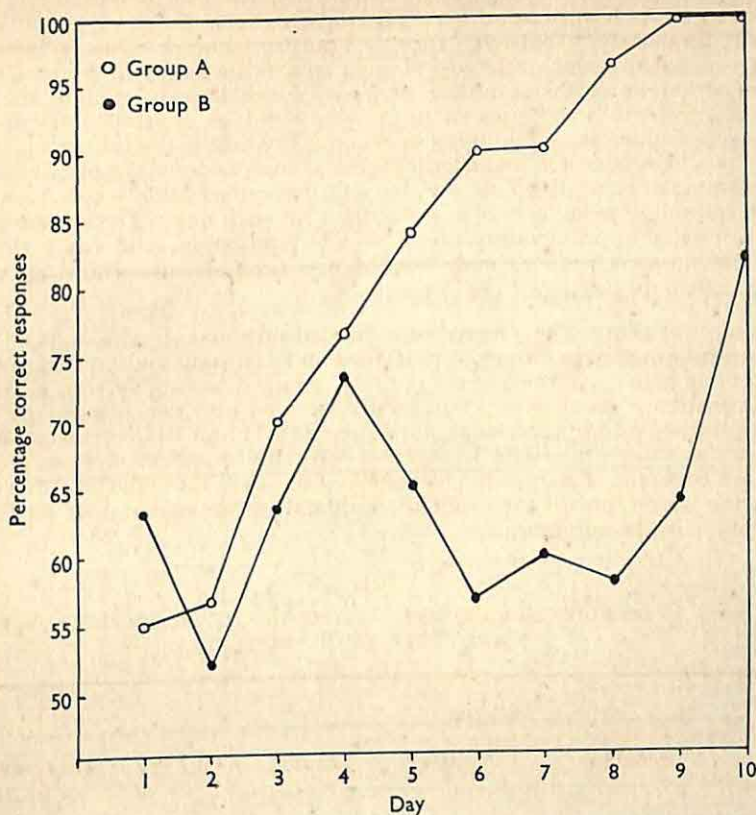
Group	Training patterns		Subgroup	Type of stand	No. of rats
	Positive	Negative			
A			AI	3-card	3
			AII	2-card	3
B			BI	3-card	4
			BII	2-card	2

All rats were trained to a criterion of 20 errorless jumps, the method of recording and scoring being one previously described (Dodwell, 1957). After attaining this criterion, each rat was given 10 transfer trials, with all doors unlocked, to square and circle. Then each rat was given 60 training trials on the discrimination of square from circle, some animals from both groups (A and B) being trained with square positive, some with circle positive. This division is shown in Table IV below.

RESULTS

A pronounced preference was demonstrated, but the preference was opposite to that predicted on Deutsch's hypothesis of biological utility. That is to say, animals trained on vertical striations positive (group A) learned more quickly than group B. Figure 2 shows the course of learning for the two groups, over the first 9 days (90 trials). Differences between the groups after the fourth day are particularly striking. An analysis of variance on the number of trials to criterion, using Fei Tsao's procedure for unequal subgroups (Ferguson, 1960) showed that the difference between horizontal

FIGURE 2



Training on striations: group A, vertical positive; group B, horizontal positive.

positive and vertical positive is significant, but the difference between stands is not (see Table IIA). Both groups showed initial preferences for their positive shapes, but they are not significant on a χ^2 test of frequency of correct jumps on the first day of training. Table IIB shows that, both in terms of number of trials and of errors, the three-card stand animals learned less rapidly than the two-card stand animals. Hence, the contention that a three-card stand is more efficient is not supported, where oddity learning is ruled out.

TABLE II
(A) ANALYSIS OF VARIANCE ON INITIAL TRAINING SCORES

Source	d.f.	s.s.	M.S.	V.R.
Between stands	1	193	193	—
Between shapes*	1	3131	3131	6.1**
Interaction	1	44	44	—
Within	8	4138	517	—
Total	11	7506	—	—

* I.e. between horizontal positive and vertical positive. An analysis of error scores yielded substantially the same result: a significant difference between shapes, but not between stands.

** $p < 0.05$.

TABLE II—continued

(B) MEAN SCORES

Group					Mean trials to criterion	Mean errors to criterion
AI	..	..	..	..	93.3	16.7
AII	..	..	..	..	73.3	12.0
BI	..	..	..	..	260.0	49.0
BII	..	..	..	..	160.0	42.0

Transfer tests. The transfer tests showed no significant overall tendency to jump to the squares. Taking the subgroups separately, however, group AII showed a significant preference for the square, but none of the others did so. See Table III.

TABLE III

TRANSFER TESTS TO SQUARE AND CIRCLE

Group					Percentage jumps to square	χ^2
AI	..	..	..	..	56.7	N.S.
AII	..	..	..	..	76.7	$p < 0.01$
BI	..	..	..	..	35	N.S.
BII	..	..	..	..	52.5	N.S.
All	..	..	..	..	60	N.S.

TABLE IV

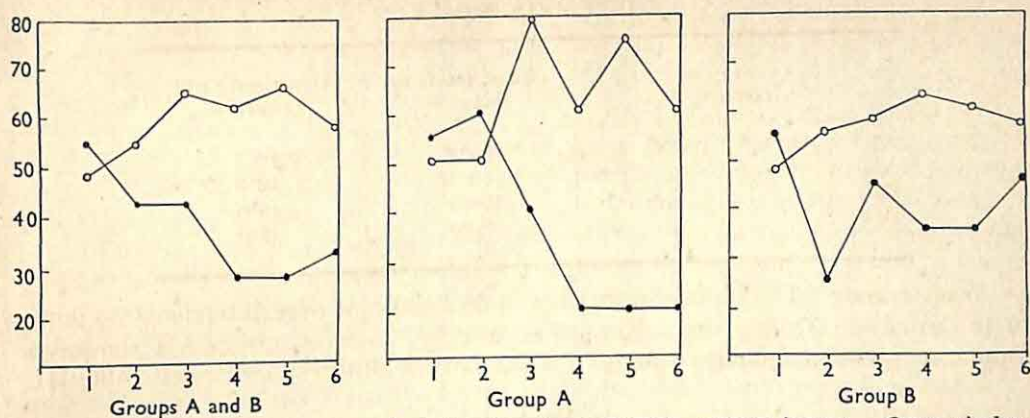
TRAINING TO SQUARE AND CIRCLE

Group	Training shapes		N	Percentage jumps to square	χ^2
	Positive	Negative			
A	square	circle	2	62.5	$p < 0.01$
B	circle	square	2	64.2	$p < 0.01$
	square	circle	4	56.8	$p < 0.05$
A + B	circle	square	2	60	$p < 0.05$
A + B	square	circle	6	59.5	$p < 0.001$
A + B	circle	square	4	62.1	$p < 0.001$

There were no significant differences between percentage jumps to the square when these were rewarded and when they were punished, nor was the overall percentage of jumps to the square significantly greater for group A than for group B.

Training on squares and circles. With the exception of two rats in group AII, each of which made nine jumps to the square (the only ones which did this) all animals were given 60 training trials on the discrimination of square from circle. Table IV shows the various conditions, together with the results of training: some animals from both groups were trained with square positive, some with circle positive. In all cases there was a significant preference for the square, even where jumps to the square were punished: Figure 3 shows the learning curves for both groups combined, and for groups A and B separately. It is particularly noteworthy that preferences do not show up on the first day of training, and seem to be maximal on about the third

FIGURE 3



Transfer training on squares and circles showing per cent. correct jumps. Open circles, rats trained with square positive; filled circles, rats trained with circle positive.

or fourth day. Thus, the tendency to jump to the square *increases* even in those rats which are punished for jumping to the square, and in the same rats the tendency to jump to the circle—a rewarded response—diminishes. Evidently a straightforward explanation in terms of habit strength and reinforcement would be impossible (cf. Dodwell, 1958 *b*, 1961). The tendency to jump to the square was about the same for the rats rewarded for this response as it was for those punished: χ^2 tests showed differences were non-significant overall, and for groups A and B separately. Group A rats tend to show more pronounced preference for the square than do those in group B (Fig. 3), but the difference is not significant. There were no significant differences in the performance of two-card and three-card stand animals.

Two rats in group AII showed, individually, significant preference for the square in the initial transfer situation. These two rats were not given training on squares and circles; if they had been, no doubt they would simply have increased the overall tendency to jump to the square in group A. Instead, they were given a second transfer test, on circle *v.* equilateral triangle with one side vertical, to see whether a vertical side was as effective as an isolated discriminandum as a horizontal side (Dodwell, 1957). The tests demonstrated no tendency to jump to the triangle.

Conclusions. The following conclusions can be drawn:—

- These rats, having had no pre-training on shape and/or orientation, show a marked preference for vertical as against horizontal striations.
- Training on discrimination of horizontal and vertical striations, whichever one is positive, leads to a strong preference for squares over circles in subsequent training.
- Both these preferences fail to show up in the first few training trials.
- The experiment does not yield evidence to support the contention that a three-card stand yields more efficient discrimination learning than a two-card stand.

DISCUSSION

Conclusions (a) and (b) effectively dispose of Deutsch's argument about the biological advantages of jumping to horizontal shapes, but there is no obvious reason why they should prefer the vertical. However, the result does reinforce the argument

put forward elsewhere (Dodwell, 1957, 1961) that orientation is of prime importance in shape discrimination for the rat. One might wonder why this result has not been obtained previously. This seems puzzling, but the reason may be that in previous work jumping to an open door has always preceded training on striations (see below).

The results of the transfer tests are at variance with results reported previously (Dodwell, 1957). However, the conditions were not identical in the two experiments; thus, in the earlier experiment, where rats showed immediate and perfect transfer to squares after training on horizontal and vertical striations (horizontal positive) the rats had previously learned to discriminate a plain wooden door on a black background from a plain black card, so that in effect they had had two discrimination situations involving a horizontal line in the positive shape before transfer tests on square and circle. Also, the shapes used in the earlier experiment were somewhat larger than those used in the present experiment. It is possible, too, that there may be some difference between strains of hooded rats in visual capacity; there were considerable individual differences in transfer preferences in the present experiment, and it will be recalled that two rats gave almost perfect transfer to the square—but after training with *vertical* striations positive.

The results of the subsequent training on squares and circles, on the other hand, are clear-cut, and might have been predicted from the writer's theory of shape recognition (Dodwell, 1957) since the square shares common elements both with the horizontal and with the vertical striations. It could not have been predicted in Deutsch's theory alone, nor with his subsidiary system for signalling information about orientation (Deutsch, 1958) nor indeed with both these two together added to the hypothesis of biological utility. In the latter two systems Deutsch would have to predict avoidance of the square in rats trained on vertical positive, since jumps to the horizontal shape (which, in Deutsch's terms is the only effective element here) had previously been punished. Another possibility is to maintain that rats have a preference for squares over circles, and that the previous training on horizontal and vertical striations is irrelevant. However, it has been shown that rats which have had no previous training on shape discrimination fail to discriminate between squares and circles of the sort used in the present experiment in 300 jumps (Dodwell, 1960), so the preferences observed can reasonably be attributed to previous experience of striations.

The fact that preferences for the square were not immediate, although very marked when they did appear, supports an argument put forward elsewhere (Dodwell, 1960) on the dangers of drawing inferences about lack of discriminatory transfer on the basis of negative results in immediate transfer tests. Why discriminatory transfer should appear only after a number of trials is something of a mystery (cf., however, Dodwell, 1961).

Thus, apart from demonstrating an unexpected fact in visual orientation preferences in the rat, the experiment has disposed of the hypothesis that transfer in terms of horizontal lines is a specially favourable case for the writer's theory, and has specifically refuted three implications of that hypothesis, (a) that horizontal lines should be preferred to vertical, (b) that learning to jump to horizontals should be accomplished more quickly than learning to jump to verticals, and (c) that there should be transfer from horizontals to squares, but not from verticals to squares.

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REFERENCES

- DEUTSCH, J. A. (1955). A theory of shape recognition. *Brit. J. Psychol.*, **46**, 30-7.
- DEUTSCH, J. A. (1958). Shape recognition: a reply to Dodwell. *Brit. J. Psychol.*, **49**, 70.
- DODWELL, P. C. (1957). Shape recognition in rats. *Brit. J. Psychol.*, **48**, 221-9.
- DODWELL, P. C. (1958 a). Shape recognition: a reply to Deutsch. *Brit. J. Psychol.*, **49**, 158.
- DODWELL, P. C. (1958 b). Visual shape discrimination in animals and men. *D. Phil. thesis, Oxford*.
- DODWELL, P. C. (1960). Discrimination of small shapes by the rat. *Quart. J. exp. Psychol.*, **12**, 237-42.
- DODWELL, P. C. (1961). Coding and learning in shape discrimination. *Psychol. Rev.* (In press.)
- FERGUSON, G. A. (1960). *Statistical Analysis in Psychology and Education*. New York.
- FIELDS, P. E. (1953). The efficiency of the serial multiple visual discrimination apparatus and method with white rats. *J. comp. physiol. Psychol.*, **46**, 69-76.
- LASHLEY, K. S. (1938). The mechanism of vision. XV. Preliminary studies of the rat's capacity for detail vision. *J. gen. Psychol.*, **18**, 123-93.

SHORTER ARTICLES AND NOTES

LIKE AND CROSS MODALITY RECOGNITION IN SUBNORMAL CHILDREN

BY

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The ability of mentally subnormal children to recognize previously presented visual or verbal stimuli was compared. In "like-modality" procedures they had to recognize words or pictures in the modality in which they had originally been presented. In "cross-modality" procedures the stimulus they had to recognize was in the opposite modality from that which had been used for the first presentation.

In every recognition test the items had to be recognized from among an equal number of novel stimuli. Cross-modality procedures resulted in higher recognition scores. The hypothesis is advanced that this could be accounted for by the necessary translation from one type of sensory image to another in the cross-modality trials, which might minimize over-generalization.

INTRODUCTION

Although it has sometimes been claimed that some subnormal patients have good memories, controlled experiments investigating their capacity to retain and recognize material under varying conditions are rare. The nature and extent of memory in the intellectually deficient is of obvious practical as well as theoretical interest. Any attempt to analyse the deficits in the thought and learning processes of the subnormal, will have to take account of the manner in which stimulus presentation and observation influence both immediate and long-term memory.

Bartlett (1932), Kurz and Hovland (1953) and others have demonstrated the value of verbalization and naming in increasing efficiency of recognition in normals. On the other hand we had previously confirmed findings of Luria and his colleagues (1956) that the subnormal have difficulties in using words as guides for voluntary behaviour. In addition, results obtained by us in a previous study (1960) must be recalled. In this study we found that imbeciles performed a task more successfully if they responded in a manner different from the one in which the stimulus was presented, than when stimulus and response were either both visual or both motor activities. No such relationship was found in a normal control group. These results were interpreted as indicating that tendencies to echolalia and echopraxia were prominent in imbecile children, if stimuli and responses belonged to the same sense modality. If, however, the subject was asked to respond differently, we supposed that the possibility for imitative behaviour was minimized and more independent behaviour occurred.

Therefore the hypothesis for the present experiment was that cross-modality recognition would necessitate translation from one type of sensory image to another. This would prevent errors in recognition, caused by simply affirming indiscriminately that any stimulus in the recognition series had occurred before. In other words, while stereotyped affirmative responses would for instance be given to a series of pictures, substitution of the corresponding words in the recognition test would favour discrimination.

METHOD

The subjects were 72 institutionalized imbeciles.* Twenty-four had I.Q.s from 32 to 45, Mean I.Q. 39, M.A. from 4 to 6 years, and 24 had I.Q.s of 40 to 55, Mean I.Q. 48, M.A. from 7 to 9 years. I.Q.s were based on the Terman-Merrill Scale. To compare short-term with long-term memory a third group of 24 imbecile children was given the recognition task 1 week after initial stimulus presentation. These were matched in age and I.Q. with those of mental age 4 to 6 years. The groups were clinically heterogeneous and only those with sensory or motor defects were excluded. In each of the two low I.Q. groups and in the higher I.Q. group quadruplets, matched for mental and chronological age were formed and the individual members randomly allocated to four sub-groups. Treatments given were: visual-visual, auditory-auditory, visual-auditory and auditory-visual.

TABLE I
STIMULUS AND CONTROL WORDS

<i>Number</i>	<i>Stimulus words</i>	<i>Percentage frequency</i>	<i>Number</i>	<i>Control words</i>	<i>Percentage frequency</i>
1	Tree	97.5	9	Girl	97.5
2	House	93.8	10	Horse	95.0
3	Ball	91.3	11	Boat	92.5
4	Clock	87.5	12	Chair	88.8
5	Train	81.3	13	Bed	81.3
6	Cow	78.8	14	Fish	78.8
7	Bird	77.5	15	Fork	76.3
8	Hand	60.6	16	Flag	61.3

The material consisted of 16 simple line drawings of common objects, on cards measuring $3\frac{1}{2}$ in. $\times$ 5 in., and a magnetic tape on which the spoken names of these objects were recorded. The latter are given in Table I. The stimuli were selected from Mein's (personal communication) vocabulary of imbecile speech, and only words which had a frequency of occurrence of 60 per cent. or over were included.

For any one sub-group eight items were presented once, singly in succession, with a 10-sec. interval between stimuli. Those presented with words were required to repeat each word as it was heard. Those receiving pictorial stimuli matched each one in turn with the identical pictures in their possession, which had been arranged in the same order as the cue stimuli. Immediately following this, these same eight stimuli had to be picked out from among an equal number of new ones in the like-modality recognition tests. In

TABLE II
EXPERIMENTAL DESIGN

<i>Group</i>	<i>1st Presentation</i>	<i>2nd Presentation</i>
1	8 Pictures	16 Pictures
2	8 Words	16 Words
3	8 Pictures	16 Words
4	8 Words	16 Pictures

cross-modality trials, stimuli were presented in the other modality for recognition. Instead of the pictures previously presented their names, and instead of words the corresponding pictures, had to be recognized from among an equal number of new words and pictures. The stimuli in the recognition tests were also presented singly in succession in random order. Instructions were minimal. Those in the like-modality groups were asked to say if they had seen or heard each item on the previous occasion. The cross-modality groups were required to say if they had heard the name of a corresponding picture, or seen the picture corresponding to a particular word at the preceding session. No corrections or reinforcements were given. The experimental procedure is shown in Table II.

RESULTS

The data are the correct "Yes" and "No" responses to each of a sequence of 16 stimuli. A chance score is taken to be at the level of 50 per cent. correct answers. This may be achieved by giving uniformly positive or negative replies, or in the more expected way by a random distribution of "Yes" and "No" answers. A transformation, suggested by A. E. Maxwell on the principle underlying Cox's (1958) method for the analysis of trends in binary sequences was adopted to give a measure for each subject of the deviation of his score from chance.

The stimuli were ranked from 1 to 16, and a score was obtained as the sum of those ranks to which a correct response was given. This process was repeated ranking in the opposite direction and the ranks again summed. The mean of the two sums of correct rankings was taken as the final score which was expressed as the positive or negative deviation of the figure obtained from a chance distribution of success or failure (i.e. half the total sum of ranks). The means and standard deviations of the transformed scores are given in Table III.

TABLE III
MEAN RECOGNITION SCORES FROM A POSSIBLE MAXIMUM OF 68

Presentation	Recognition	Immediate recognition		Long-term recognition M.A. 4 to 6
		M.A. 7 to 9	M.A. 4 to 6	
Auditory	Auditory	51.0 $\pm$ 24.5	9.0 $\pm$ 9.7	8.5 $\pm$ 12.2
Visual	Visual	49.9 $\pm$ 25.5	17.0 $\pm$ 21.9	1.3 $\pm$ 3.2
Auditory	Visual	43.8 $\pm$ 21.1	31.1 $\pm$ 24.3	18.4 $\pm$ 18.8
Visual	Auditory	60.8 $\pm$ 9.2	43.9 $\pm$ 23.8	19.8 $\pm$ 21.2

A three-way analysis of variance comparing higher with lower mental ages, like- with cross-modality, and visual with auditory recognition was carried out on the transformed scores. A significant difference between mental age groups was found, those with higher mental ages performing better throughout. There was also a significant difference between like- and cross-modality recognition scores, showing that cross-modality trials resulted in better performance. In the higher mental age group all subjects obtained relatively high scores, so that differences between sub-groups are not very marked. Those with lower mental ages obtained the best results under conditions of visual presentation and auditory recognition, and the auditory-auditory sub-group had the lowest scores. Results of the analysis are given in Table IV.

TABLE IV
ANALYSIS OF VARIANCE RESULTS

Source	V.R.	d.f.	F	P
Mental age	8164.08	1	17.37	<0.001
Vis.-aud. recognition	402.52	1	0.86	N.S.
Like-cross mod. recognition	2133.33	1	4.54	<0.05
M.A. $\times$ Vis.-aud. recognition	140.09	1	0.30	N.S.
M.A. $\times$ Like-cross mod.	1507.53	1	3.21	N.S.
Vis.-aud. $\times$ Like-cross	990.09	1	2.11	N.S.
M.A. $\times$ Vis.-aud. $\times$ Like-cross	20.00	1	0.04	N.S.
Error	469.95	40		

There was an interesting indication of the nature of errors in the memory process. More errors were due to giving a positive recognition response to a stimulus which had not previously been presented than to failure to recognize one of the positive items. This was particularly apparent with subjects of lower mental age. However, when the results of those with lower mental ages were compared under like- and cross-modality conditions, 8 out of 12 in the like-modality groups gave either randomly distributed or indiscriminately affirmative responses, thus obtaining a score of 0, while only 3 out of 12 in the cross-modality groups had such scores ($\chi^2 = 4.2$, $p = 0.05$).

Long-term recognition

The same tendencies which are apparent in immediate recognition are present after a week's interval, although their effect has become less marked. Table III shows the mean long-term recognition scores. An analysis of variance between like-cross and visual-auditory modality showed that neither the main effects nor the interaction were significant, although the effects of like- versus cross-modality comes near the 0.05 level. Again 8 out of 12 children in the like-modality groups gave uniform stereotyped affirmative recognition responses which gave them a score of 0, but 6 out of 12 also gave such responses in the cross-modality group.

DISCUSSION

The results presented support the hypothesis stated earlier. Cross-modality recognition in immediate memory proved more efficient than like-modality recognition. The effect is weakened after a time interval.

The explanation for the success of cross-modality conditions may depend partly on the phenomenon of echolalia noted for example by Kent (1910) as frequent in defectives. In association tests, defectives tend to respond with the last word of their interrogator. Because of this tendency their capacity for making independent responses is temporarily inhibited.

The failure to inhibit responses to negative cues was also observed by Zeaman and his colleagues (House, Zeaman, Orlando and Fisher, 1957). A number of visual discrimination studies carried out by these investigators showed that responses to positive cues were learned faster than the avoidance of responses to negative stimuli. Indeed, Zeaman *et al.* state that avoidance tendencies to negative cues may be absent in some imbeciles. The present results seem to show that such behaviour may depend on the nature of the task as well as on the subject.

Failure to recognize may also be due to impairment of the ability to see relevant differences between stimuli. Lashley and Wade (1946) describe such primitive "generalization" as due to such a failure, and distinguish this from true generalization which is based on perception of both similarities and differences. In our like-modality trials all words or all pictures, regardless of their specific characteristics were perceived as having occurred before. The necessity to "encode" verbal into visual stimuli and vice versa in the cross-modality conditions may make the association between word and image more exact.

In a task which is difficult for them these subjects tend to give stereotyped affirmative answers. Consequently the cross-modality method should be regarded as reducing interference rather than improving recognition. This may be relevant to observations which tend to attribute learning difficulties in defectives to tendencies to rigidity, perseveration and stereotyped behaviour. But probably such behaviour is generally evident in animals as well as humans whenever a task is too difficult.

The present results suggest that perseveration is at least partly dependent on the task and even when it exists as an independent tendency it can be increased or decreased by variation in the circumstances under which the response is demanded.

We would like to thank Dr. J. M. Crawford, Physician Superintendent of Botleys Park Hospital, and Dr. L. T. Hilliard, Physician Superintendent of the Fountain Hospital, for granting us facilities to carry out this work. We would also like to thank Miss Atholl Hughes, who helped us with testing.

REFERENCES

- BARTLETT, F. C. (1932). *Remembering*. London.
- COX, D. R. (1958). Regression analysis of binary sequences. *J. Roy. Stat. Soc. (B)*, **20**, 216-42, Example 3, p. 225.
- HERMELIN, B., and O'CONNOR, N. (1960). Like- and cross-modality responses in normal and subnormal children. *Quart. J. exp. Psychol.*, **12**, 48-53.
- HOUSE, B. J., ZEAMAN, D., ORLANDO, R., and FISHER, W. (1957). *Learning and Transfer in Mental Defectives*. The University of Connecticut, Department of Psychology, Progress Report No. 1.
- KENT, G. H., and ROSANOFF, A. J. (1910). A study of association in insanity. *Amer. J. Insan.*, **67**, 37-96, 317-90.
- KURZ, K. H., and HOVLAND, C. I. (1953). The effect of verbalization during observations of stimulus objects upon accuracy of recognition and recall. *J. exp. Psychol.*, **45**, 157-64.
- LASHLEY, K. S., and WADE, M. (1946). The Pavlovian theory of generalization. *Psychol. Rev.*, **53**, 72-87.
- LURIA, A. R. (Ed.) (1956). *Problems of the Higher Nervous Activity of Normal and Abnormal Children*. Moscow. Vol. I and II, 58.

A NOTE ON THE INFLUENCE OF DIFFERENT ORDERS OF APPROXIMATION TO THE ENGLISH LANGUAGE UPON EYE-VOICE SPAN

BY

EVERDINA A. LAWSON

From the Psychological Laboratory of the University of Hull

An experiment is described in which the effect of changing information content of stimuli upon the eye-voice span in reading is investigated. Subjects were required to read aloud statistical approximations to English. It was found that the eye-voice span is inversely related to the information content of the material read. This would support the suggestion of a constant rate of information transmission in reading, since the different orders of statistical approximations differ in the amount of redundancy present in the material.

INTRODUCTION

From the work of Goldmann-Eisler (1958) on the relationship between pauses in spoken languages and the information content of the word before which the pause occurred, and from that of Aborn, Rubenstein, and Sterling (1959) upon contextual constraint in sentences, it seemed possible that Fairbanks' (1937) early work on the eye-voice span could be further quantified.

METHOD

The different orders of statistical approximations to English compiled by Taylor and Moray (1960) were used. They were presented to the subject on a continuous paper tape. After some practice runs with ordinary prose, the experimental material was presented in decreasing order of approximation. That is, first 16th order, then 12th order, etc., were presented. The tape on which the material was written was pulled slowly past the subject who was requested to read aloud, at his ordinary reading speed. At a predetermined word, which was the same for all subjects, the light was switched off and the subject was requested to report as many words as he had seen but not already pronounced. A control against guessing instead of reporting perceived words was obtained by presenting the same material up to the same predetermined words, but preventing the subject from seeing any of the subsequent words. Different subjects were used for this control experiment. In order to obtain an idea whether the intersubject variation of reading speed had some influence upon the eye-voice span, the subjects' reading speed was measured on two samples each of 50 words of 8th, 6th, 5th, 4th, 3rd, and 2nd order of approximation to English (Deese's material) and also upon 50 words of light prose.

Subjects were 12 undergraduates from the psychology department, eight of which were used for the actual experiment and four for the control experiment.

RESULTS

The means and standard errors and the results of an analysis of variance are given in Tables I and II. The analysis of variance was performed on the assumption that the number of words can be taken to form an interval scale. However, since this

TABLE I

	16th	12th	8th	6th	4th	2nd	Order
Mean ..	4.63	4.67	4.52	3.86	3.54	3.52	number of words
S.E. ..	2.25	2.46	2.6	1.9	0.64	1.6	$n = 8$

TABLE II
ANALYSIS OF VARIANCE OF DATA IN TABLE I

		<i>s.s.</i>	<i>d.f.</i>	<i>Variance</i>	<i>F</i>	<i>p</i>	<i>Error term</i>
Column (material)	..	125	5	25	13.23	<0.001	1.89
Rows (subjects)	..	75.2	7	10.74	7.41	<0.001	1.45
Interaction		66.3	35	1.89	1.303	> 0.05	1.45
Residual		348	240	1.45			

assumption might be unwarranted, considering the varying transition probabilities, a Friedman two-way analysis of variance was performed. $\chi^2_r = 28$ (*d.f.* = 5) was found, which is significant beyond the 0.001 level as $\chi^2_r = 20.51$ at the 0.001 level for *d.f.* = 5. If the results of the control experiment are treated in the same way as the results of the actual experiment, that is, only correct words are being scored, then none of the four subjects made a score at all on any of the six different orders, and here no statistical treatment is given. The pooled data on reading speed do not show regular change with order of approximation, although in the case of two subjects there was evidence that reading speed declined with increase in information content. If we regard the subject as processing information content at a constant rate, it appears that this is achieved by lowering the eye-voice span rather than rate when the information in the stimulus increases.

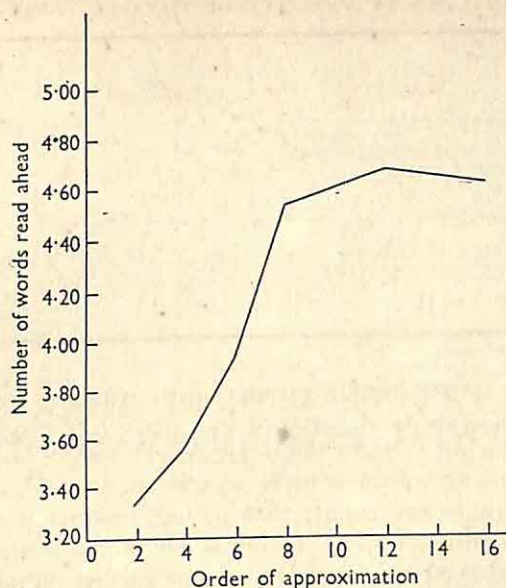
DISCUSSION

The hypothesis that the eye-voice span should decrease when the information content of the material increases, appears to be supported by the experiment. This is related to the report by Fairbanks (1937) who, with an eye-voice camera recorded the exact eye movements, and noticed that the eye-voice span might reduce to almost zero for difficult words. The results of this experiment appear to agree with the findings by Sumby and Pollack (1954). They instructed subjects to reproduce material by writing with the least possible number of glances. They found that a glance was necessary for every three words in zero order language, but only one for every 10 words in ordinary prose. These authors arrive at the following conclusion: "that when sequential restrictions are varied with transmitted materials, the rate of information transmission remains fairly constant," a conclusion supported by the experimental results obtained here.

The fact that the change of number of words read ahead with increased order of approximation seems to occur mainly up to the eighth order of approximation, might be explained by the observation that the number of words read ahead, is below eight words even in the highest order of approximation. That is, at the eighth order the effect on the eye-voice span seems to reach a limiting value (Fig. 1). This fits in with the work of Aborn, Rubinstein and Sterling (1959), who concluded that the maximal effectiveness of context lies between 5 and 10 words.

The writer wishes to thank Mr. G. Westby for providing laboratory facilities; Mr. N. Moray for supervising the experiment; and the Hull Education Authority for the provision of the financial assistance necessary for the experiment to be carried out.

FIGURE 1



REFERENCES

- ABORN, M., RUBINSTEIN, H., and STERLING, T. D. (1959). Sources of contextual constraint upon words in sentences. *J. exp. Psychol.*, **57**, 171-80.
- FAIRBANKS, G. (1937). The eye-voice span in oral reading. *Psychol. Monogr.*, **48**.
- GOLDMAN-EISLER, F. (1958). Speech production and the predictability of words in context. *Quart. J. exp. Psychol.*, **10**, 96-106.
- SUMBY, W. H., and POLLACK, I. (1954). Short-term processing of information. *HFORL Report Tr.* 54-6. January.
- TAYLOR, A., and MORAY, N. (1960). Statistical approximations to English and French. *Language and Speech*. **3**, 7-10.

EXPERIMENTAL PSYCHOLOGY MONOGRAPHS

The Experimental Psychology Society propose to launch a series of monographs under the editorship of the Editorial Board of this *Journal*. The monographs will be published by Messrs. W. Heffer & Sons, Ltd., Cambridge, England, and will appear occasionally.

Experimental Psychology Monograph No. 1 is in press and should be available shortly. Its title is *The Methods and Findings of Experiments on the Visual Discrimination of Shape by Animals* and the author is N. S. Sutherland, Oxford University. Full details of this publication will be given in our next issue.

The Editorial Board will be glad to receive and consider original contributions to experimental psychology and related disciplines suitable for publication in monograph form. Manuscripts, which should not exceed 45,000 words, should be prepared in accordance with the conventions of this *Journal* and addressed to the Editor, *Quarterly Journal of Experimental Psychology*, Psychological Laboratory, Downing Place, Cambridge, England.

PROCEEDING OF THE EXPERIMENTAL PSYCHOLOGY SOCIETY, 1960

28th-30th March, 1960. Extended meeting at Oxford.

1st Session: "Information and reaction time," a symposium by J. A. Leonard, M. Stone (by invitation), R. Conrad, D. E. Broadbent and M. Gregory.

2nd Session: "The localization of intellectual functions," by J. McFie (by invitation). "Cerebral dominance and the organization of simple motor skills," by A. Elithorn, M. Kerr and R. Mingay.

3rd Session: "Some aspects of aggressive behaviour in wood ants," by L. Newman (by invitation). "Immediate recall and word frequency," by R. Corteen (by invitation). "Rotary nystagmus as a means of assessing cone threshold in animals," by N. Loveless (by invitation). "A study of the processes involved in the changes of strength of an instinctive response after exposure to a constant stimulus," by R. A. Hinde.

4th Session: "Some temporal and structural properties of behaviour during conditioning and extinction," by H. M. B. Hurwitz and J. Millenson. "Conditioning in earthworms with and without cerebral ganglia," by S. C. Ratner. "An information processing theory of verbal learning," by E. A. Feigenbaum (by invitation).

5th Session: Start of Joint Meeting with the Society for the Study of Animal Behaviour. "Visual perception and orientation in locusts," by G. K. Wallace. "Some experiments on interocular transfer in octopus," by W. R. Muntz. "Recent findings on shape discrimination in octopus," by N. S. Sutherland. "Eye movements and colour vision in man," by D. H. Fender.

11th-13th July, 1960. Extended Meeting at Cambridge.

1st Session: "Completion of visual forms across hemianopic field defects," by E. War-
rington (by invitation). "The Cambridge Psychological Laboratory," by O. L. Zang-
will.

2nd Session: Short Communications by K. W. Blyth (by invitation), A. Watson, B. Birand
and G. C. Grindley.

3rd Session: "Frontal stimulation and immediate memory in the monkey," by L. Weis-
krantz, C. Gross and L. Mihailovic. "Non-parametric scaling," by J. W. Whitfield.

4th Session: "Reading, context and the perception of words," by J. Morton (by invitation).
"Verbal cues and selective listening," by A. Taylor (by invitation).

5th Session: "Feedback and understanding in process control," by E. R. W. Crossman.
"The perception of movement," by R. L. Gregory.

4th-5th January, 1961. 13th Annual General Meeting at Birkbeck College, London.

1st Session: "Response instruction and human discriminative behaviour," by R. J.
Audley and C. P. Wallis. "Eye/hand laterality in transfer of a motor skill," by
A. A. G. Walbridge (by invitation). "Sequential dependencies among letters and
the learning of nonsense syllables," by A. Baddeley (by invitation).

2nd Session: "The electrical activity of an irrelevant muscle group during the performance of a muscular control task," by A. J. Benson, H. M. Ferres and J. L. Gedye (by invitation). "Some experiments on immediate memory," by A. F. Sanders (by invitation).

3rd Session: "Choice reaction times and the theory of intermittency in skilled performance," by R. Davis. "Localized stimulation in the brain of the honeybee," by D. Vowles.

4th Session: "An application of Fourier Analysis to flicker fusion," by K. Gibbins and C. I. Howarth. "A theory of selective listening," by N. Moray. "Conduction delay and the judgment of simultaneity," by A. M. Halliday and R. Mingay.

Committee, 1961: President: Professor H. Kay.

Editor: Professor O. L. Zangwill.

Ordinary Members of Committee: Dr. R. J. Audley (Asst. Hon. Secretary), Dr. J. Brown (Hon. Secretary), Mr. B. M. Foss, Dr. A. M. Halliday, Dr. R. Harper, Dr. C. I. Howarth, Dr. E. C. Poulton, Dr. N. S. Sutherland, Dr. J. Szafran (Hon. Treasurer).

The following have been elected into membership of the Society:

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N. F. Dixon, Ph.D., Department of Psychology, University College, Gower Street, London, W.C.1.

P. C. Dodwell, D.Phil., Department of Psychology, Queen's University, Ontario, Canada.

J. T. Eayrs, D.Sc., Department of Anatomy, The University, Birmingham, 15.

N. E. Loveless, Ph.D., Department of Psychology, St. Andrew's University, Queen's College, Dundee.

R. Melzack, Ph.D., Department of Psychology, Massachusetts Institute of Technology, Cambridge 39, Mass., U.S.A.

T. R. Miles, M.A., Department of Philosophy, University College of North Wales, Bangor.

G. H. Mowbray, Ph.D., Applied Psychology Laboratory, Johns Hopkins University, Baltimore, Maryland, U.S.A.

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H. Tajfel, M.A., Department of Social Administration, The University, Oxford.

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E. Warrington, Ph.D., Institute of Neurology, National Hospital, Queen Square, London, W.C.1.

T. C. D. Whiteside, M.B., Ch.B., Ph.D., R.A.F. Institute of Aviation Medicine, Farnborough, Hants.

M. Wyke, Ph.D., Institute of Neurology, National Hospital, Queen Square, London, W.C.1.

Honorary Membership: Sir Frederic Bartlett, F.R.S., has been elected an Honorary Member.

Visiting Foreign Members:

E. J. Archer, Ph.D., A.P.R.U., 15, Chaucer Road, Cambridge.

J. E. Birren, Ph.D., Psychological Laboratory, Downing Street, Cambridge.

R. M. Boynton, Ph.D., National Physical Laboratory, Teddington, Middlesex.

I. Pollack, Ph.D., A.P.R.U., 15, Chaucer Road, Cambridge.

BOOK REVIEWS

Psychoanalysis, Scientific Method and Philosophy. Edited by Sidney Hook. New York University Press. 1959. Pp. xiii + 370. \$5.00.

This book contains the papers read at the Second Annual Meeting of the New York University Institute of Philosophy. *Part I: Psychoanalysis and Scientific Method* consists of papers by Heinz Hartman, Former President, International Society for Psychoanalysis, Ernest Nagel, Professor of Philosophy, Columbia University, and Lawrence S. Kubie, Clinical Professor of Psychiatry, Yale University School of Medicine. *Part II: Psychoanalysis and Society*, consists of papers by Abram Kardiner, Professor of Psychiatry, Columbia University, Ernest van den Haag, Adjunct Associate Professor of Sociology, New York University, and Alex Inkeles, Professor of Sociology, Harvard University. *Part III: Psychoanalysis and Philosophy*, consists of papers by three professors of philosophy, Morris Lazerowitz, Donald Williams and Anthony Flew. *Part IV* contains further discussion and comments on what has gone before by other contributors.

An interesting and fortunate feature of the book is the way it is self-correcting.

For instance Lazerowitz says much which gives the impression that he thinks that those philosophers who have made paradoxical pronouncements, e.g. "Time is not real," are not making statements as to matters of fact, and are not making logical statements, nor statements as to how words are in fact used, but are introducing linguistic innovations mainly for the purpose of expressing hidden propositions "important for our unconscious mental life" (p. 153). But when we read Flew we are reminded of how much philosophers have tried to show us something of a logical kind and have succeeded. They have drawn our attention to the differences between the procedure appropriate to a question as to a matter of fact, a question of logic, a question of morals, a question of philosophy and so on. What philosophers remark is always something we knew already. But this doesn't mean that what they remark is not worth remarking, that they never show us something we hadn't realized. We knew the nature of primes before Euclid proved that there is no greatest prime. On the other hand Hospers (336-9) points out some of the confusions and misinterpretations among Lazerowitz's critics. I should have thought it undeniable, though still worth pointing out, that unconscious influences play *less* part in causing differences between natural scientists and mathematicians than they play in causing differences between philosophers and poets. A more interesting question is where and how do these unconscious influences act. Lazerowitz has the courage to have a go at this. As we all know any attempt to trace the unconscious influences behind anything is apt to annoy, to infuriate, some people, and to delight others. The delight, of course, may be malicious or mischievous, or delight in discovery, or a mixture of these. The fury may be prompted by the truth of the interpretation or its falsehood or both by its truth in one respect and its falsehood in another. Hunting unconscious influences is a dangerous game but exciting and not to be abandoned because of shouts from the side lines that it's not like halma.

Nagel and Hook are inclined to think that in the case of some (many?) psychoanalytical theories, including some fundamental ones, no sufficient indication has been given of what sort of phenomena would confirm them, of what sort of phenomena would be adverse to them, and of what would be enough to show them to be false. They suspect that nothing would induce those who hold these theories to admit that they are false. A person who says "They're all against me" may show an ingenuity in explaining evidence which apparently conflicts with his theory, which finally persuades us that nothing would induce him to admit he was wrong, and that he is crazy. Arlow, Salmon and Hospers, in my opinion, do much to meet these charges against psychoanalysts. Their papers are short but excellent and should help anyone who wishes to go into the matter for himself by finding out what psychoanalysts write or say in face of these phenomena or those.

I mention here one or two rather familiar points: (i) As Hospers says it is untrue that Freud and psychoanalysts never withdraw theories, or interpretations in a particular case, in the light of evidence to the contrary. It is just untrue. (ii) A person may be VERY obstinate, VERY ingenious in defending a statement, e.g. "Whenever apparently similar circumstances lead to different results there is always a difference that makes the difference" without being crazy or talking nonsense. Whether he is saying anything worthwhile or not depends on what *sort* of ingenuity he shows. Even when a form of words, e.g. "Every movable pulley which does not give a mechanical advantage of 2P suffers from

friction" have come to express a "tautology," the fact, if it be a fact, that that tautology is "applicable" to nature tells us something about nature. (iii) A question may be a question as to what in fact is so even when it calls for little or no *further* observation or experiment, e.g. "Was this negligence or was it not?" "Is this man Newton, with his theory of gravitation, right or not?" (iv) What a person means by a new word or by an old word used in a modified sense is often best ascertained not by asking him for a definition or any account in words of what he means by his word but by noticing his practice, noticing when he applies the word and when he does not—as a child notices when people call a thing a "dog" and when not, when they call a person "silly," "mad," "sarcastic" and when not. (v) Not only psychoanalysts but poets, novelists, and people in every day life, quite often come upon situations to which the words "love," "hate" and many other words descriptive of our feelings, wishes, beliefs, are neither quite inappropriate nor quite appropriate. The words nevertheless are meaningful and very valuable for presenting the shadowy continuum of the soul.

Pap raises an old and fundamental point. He admits that many "emotion words" are partly dispositional and that introspection can go wrong here. He also says, however, "But one cannot, in the ordinary sense of the word, fail to be aware of one's hatred or of one's wish, when one tries to detect it introspectively."

Here again I offer one or two brief and inadequate comments: (i) Surely even before Freud, Dostoevsky and Schopenhauer there was some tendency to speak of someone as hating so and so, falling in love, wishing for this or that, even though he insisted that he did not hate so and so, was not falling in love, was not wishing for this or that. Pap exaggerates the rigidity of normal, pre-psychoanalytic, usage of words descriptive of what is going on in a mind. (ii) Pap speaks as if to give a psychological word or phrase a more *dispositional* use is always to give it a more *behaviouristic* use. (William James on Emotions, Russell in *The Analysis of Mind*, Ryle in *The Concept of Mind* give the same impression.) But to give a sentence of the sort "There is in X's mind hatred (love, contempt) for Y" a use in which the truth of what is said is now less a matter of whether at the moment, upon reflection, it appears to X that he has or appears to X that he has not hatred (love, contempt) for Y, and more a matter of how it would feel, if this or that were to happen, is not to make the word "hatred" ("love," "contempt") no longer descriptive of a mental state. Whether or no in the future or under the circumstances indicated X does feel as the speaker claims he will or would feel is still a matter as to which X will or would "have a say" as no one else will or would. That is the statement is still not equally open to the verification or any two observers in the way that a statement about the physical, "public" world is. If a child takes my question "Is there a cat in the room?" as one to which the answer should be "Yes" if it then and there appears to everyone that there is a cat there and one to which the answer should be "No" if it then and there appears to everyone that there is no cat there, then I have to explain to that child that my statement is related to appearance in a less simple manner. I say, "There is indeed no cat to be seen at the moment but wait," and I then call "Tabitha" and a cat emerges from behind the curtains and I say, "There I was right. There was a cat in the room," or a cat does not emerge and I pull the curtains, look under the sofa, behind the screen, and still find no cat, and say, "There *now* I must admit I was wrong." When I do all this I profoundly modify the child's understanding of the words "There is a cat in the room" and that not merely in the way I do when I teach him that "cow" means "*female* bovine animal." I am not merely teaching him that a word's usage is not as wide or as narrow as he had thought and correcting his beliefs as to how words are related to conceptions he already has. I am introducing to him the conception of a hidden cat, the conception of the third dimension. But I am not making the statement "There is a cat in the room" no longer one about the physical world. Nor do I, as some have suggested, begin to make the physical spiritual if I begin to speak of cats, currents, fields, particles, no one ever sees. For I leave the statement "There is a cat here" still such that no observer has a way of verifying it, or any part of it, which some other observer could not have. If someone modifies the use of "There is in X's mind hatred of Y" so that X's introspection at the time is not given its normal weight he may still give it a use in which X's subsequent introspection, what appears later in X's mind, is still what finally settles the truth of the statement. Psychoanalysts after all pay very great attention both to what comes into a patient's head before an interpretation has been given and also to what comes into his head after an interpretation. A statement about hidden hate is still a statement such that an observer has a way of verifying it which no other could have. Bridgman in about 150 words (p. 282) makes two penetrating comments. He writes "... in spite of the

apparently unsatisfactory status of some of the constructs of the psychoanalyst it would appear that there is nothing fundamentally unsound at the foundations, but that if one takes the necessary trouble and care everything can be put on a completely operational basis" and he adds "... perhaps some of the features which distinguish psychoanalysis from other disciplines can be explained by the prominent role that introspection plays."

Among those who in this book write about the therapeutic value of psychoanalysis is Michael Scriven. He explains to psychoanalysts that unless they use control groups and find the ratio of those cured to those not cured and what the spontaneous recovery rate is and so on then they have no scientific grounds for their beliefs, not even for their beliefs as to the help they have given to patients. He finishes by saying "without any attempt to deal with the difficulties mentioned, how in the name of Roger Bacon could a psychoanalyst imagine that his own hopelessly contaminated, uncontrolled, unfollowed up, invalidated, unformalized estimation of success has ever established a single cure as being his own work?" (p. 249). I, myself, often imagine that my conversation with someone, whether in philosophy or not in philosophy, has done harm, or again, that it has done good. My estimate is hopelessly contaminated, uncontrolled, unfollowed up, invalidated, unformalized. Nevertheless it is, I believe, often well justified. Isn't the position this? (i) The experiments and recordings which Scriven recommends would be very valuable. Indeed without them we have inadequate justification for a *general* claim such as "Any person in a bad state of mind who is given psychoanalytic treatment is more likely to be better in 4 years time than one who is given no treatment" or "than one who is given 'electrical' treatment." (ii) The experiments and recordings which Scriven recommends would not be irrelevant to a claim that in a particular case psychoanalysis would do harm (or good). (iii) They would not be irrelevant to a claim that in a particular case psychoanalysis *has* done harm (or good). But none of this implies that without Scriven's experiments and recordings one never has ample justification for saying, "Here psychoanalysis has done harm" or "Here psychoanalysis would do harm" or "Here psychoanalysis has done good" or "Here psychoanalysis would do good." Scriven is not the first philosopher to tell us that we have no real justification for beliefs of a certain kind because our justification is not upon the pattern he, in his philosophical moments, has come to think the only possible one.

JOHN WISDOM.

Behaviour and Evolution. Edited by Anne Roe and G. G. Simpson. London. Oxford University Press (for Yale University Press). 1958. Pp. viii + 557. 80s.

This symposium springs from two conferences held in America during 1955 and 1956. The aim was to bring together anatomists, physiologists, palaeontologists, ethologists, psychologists, sociologists, embryologists, entomologists, and geneticists, with a mutual interest in evolution, in the hope of mutual stimulation. None of the discussions are printed (a welcome relief), but the papers were revised in the light of the criticisms.

The organizers cast their net so wide that it is not surprising to find that the weight of the catch broke their line. Although all the headings of the papers come under evolution, there is, in fact, no common theme running through the book, and it appears that as yet the theories of evolution do not provide the unifying concepts which have been so valuable in zoology. Indeed, the most significant papers come more under the broad heading of zoology than psychology, as, for example, in the papers by Hinde and Tinbergen, Mayr, Spieth and Pittendrigh, where the authors are dealing with the dynamics of the mechanisms of behavioural evolution. The more psychologically orientated papers seem still to be trying to provide a conceptual framework from which to work—see, for example, the papers by Nissen, on Axes of Behavioural Comparison, or Thompson, on Social Behaviour. It is almost as if the swing from Behaviourism has brought the psychological study of behaviour into a maze from which methodology alone cannot lead us.

The standard of the papers varies over almost as wide a range as their subjects. At one extreme, Pribram provides a novel and extremely well argued case for reconsidering classical ideas on the evolution of the brain, which should have important consequences for behaviour. At the other, Carpenter supplies a card index of references on territoriality, which is doubly disappointing from a worker of his calibre. Harlow's contribution on The Evolution of Learning, is remarkable both for its elegant style, "It is possible that our behaviour tests have never done justice to the Turtle; that a latent imagination has been obscured by an introverted personality," and for stressing how uncomparative most comparative studies of learning have been, making a paper with his title virtually impossible to write.

It is perhaps the patchiness of the contributions which is the book's most irritating feature. One could wish that the editors had been clearer in their own minds whether the papers were intended for specialists in their own fields, or as an introduction to other people's work. Both approaches are needed, but surely not in the same volume.

D. M. VOWLES.

The Fifth Mental Measurements Yearbook. Edited by Oscar K. Buros. New Jersey. The Gryphon Press. 1959. Pp. xxvii + 1292. \$22.50.

This book, like its four predecessors, is an invaluable reference book for anyone actively interested in tests and books concerned with mental measurement. It lists and reviews every reputable psychological test, in addition to some which do not enter this category. Both tests and relevant books are usually reviewed by at least two independent, highly competent reviewers and often by more than two.

The *Yearbook* is no vehicle for a large mutual admiration society, as may be seen from the following excerpt, for instance, concerning the *Occupational Interest Inventory*: "It is very frustrating to this reviewer, in considering the possible impact of this review, to find that an inventory which received very unfavourable notices in both the third and fourth *Mental Measurements Yearbooks* is still in existence, has been researched and revised so little, and is still being offered for sale as a valid instrument." Most of the reviews of tests have been written specially for the *Yearbook*, but many of the book reviews are drawn from standard English and American journals.

The variety of qualities thought fit to test is intriguing. There are, for example, three on *Etiquette*, one on *Clothing* and one *Faculty Morale Scale for Institutional Improvement*.

Considering the immensity of Buros's task and the size of the book, it is surprisingly easy to find one's way about it. The Preface includes an instructive section on this. In addition there are five very useful Indexes: Periodicals Directory, Publishers Directory, Index of Titles, Index of Names (including authors of reviews as well as of tests and books) and a Classified Index of Tests. The book is comprehensive and up to date.

A. W. HEIM.

The Onset of Stuttering. Research Findings and Implications. By Wendell Johnson and Associates. Minneapolis. University of Minnesota Press. London. Oxford University Press. 1959. Pp. ix + 243. 40s. od.

Dr. Johnson of the University of Iowa and his Associates report in detail in this book the results of a series of three investigations into stuttering. During the last 25 years they have compared, in very many respects, altogether 246 stuttering children with 246 non-stuttering children and their parents. The large number of negative findings is perhaps of as much interest as the positive findings. No differences were found, for instance, for either of the children or their parents, on any of the items concerning handedness. There were no differences in the mean ages, as reported by the mothers, at which the various milestones were passed. On the other hand, the parents, and especially the mothers, of the stutterers tended to rate the speech and social development of their children as slower than average, and they expressed more dissatisfaction with their children and their spouses. These parents, who tended to be older than those of non-stutterers, also tended to subject their children to more pressure in training. They tended to start toilet training earlier, and to go over from bottle to cup feeding earlier. Their methods tended to be more coercive or punitive. They tended to fuss and worry more. A significantly large proportion of the stutterers were either the only or the oldest children in their families.

Stuttering first arose as a problem as a rule between the second and fourth birthday, most commonly in the fourth year of life. In only a very few cases did the onset of stuttering bear a relation in time to an illness or injury, a shock or a fright or any unusual circumstance. Cases of stuttering were found to be several times commoner in the families of stutterers than in those of non-stutterers.

The investigations were very extensive; thus the interviews with each of the parents covered more than 800 items. They have served a useful purpose because the results focus attention on the areas which now require intensive study. Stuttering, it appears, begins with the hearer.

D. RUSSELL DAVIS.

Psychology and Human Performance: An Introduction to Psychology. By Robert M. Gagné and Edwin A. Fleishman. New York. Holt & Co. 1959. Pp. xiii + 493. \$7.25.

This is an elementary textbook of experimental and applied psychology written particularly for the prospective occupational psychologist or "human engineer." It contains the usual material found in textbooks of psychology, but the authors have devised a refreshingly new layout in which each topic is presented as a contribution to the understanding of human performance at work. Only sufficient background is included to make this approach intelligible and no mention is made of psychopathology or personality except in relation to personnel selection.

The first half of the book is devoted to the cognitive processes and learning; here the authors make effective use of simple flow-diagrams to clarify the relationships between the different functions such as discrimination and habit-formation. There are chapters on motivation and social behaviour, and the book concludes with a large section on applications in personnel selection, training and job design.

The treatment is positive and factual throughout, but no attempt is made to reveal the controversial issues (e.g. in learning and instinct) or the competing theoretical approaches (e.g. in perception or group dynamics). Rather surprisingly, too, since the book should appeal to those with an engineering outlook, no mention is made of the recent work on feedback and information theory. The authors implicitly adopt the orthodox Stimulus-Response framework with the generalized drives, hunger, thirst and sex, and they leave certain facts which do not fit into this scheme unassimilated. For instance, they cite Harlow's work as demonstrating the existence of a "contact-comfort" drive in monkeys, which is plainly of a different order from the other three drives but they do not discuss the difference. Since no mention is made of the work of European ethologists, the reader is left with the impression that "contact-comfort" is somehow a single exception that proves the rule. Thus the student who proceeds straight from this book to the original literature to seek further information is likely to be badly misled on several subjects, for want of guidance on the differing viewpoints and aims of the different researches.

The book as a whole can be recommended for students taking psychology as a subsidiary subject or in diploma courses, and for engineers or designers needing a general introduction to applied experimental psychology. The later chapters should prove most useful to students of applied psychology or ergonomics.

E. R. F. W. CROSSMAN.

The Neuropsychology of Lashley. Selected Papers of K. S. Lashley. Edited by F. A. Beach, D. O. Hebb, C. T. Morgan and H. W. Nissen. London. McGraw-Hill Book Co. Inc. 1960. Pp. xx + 564. 74s. od.

The Editors tell us that their aims have been "to choose a group of papers that would reflect the range of Lashley's interests and achievements, to avoid duplication, and to hold the size of the final product within reasonable limits." By and large, they have succeeded well on all counts. The thirty-one papers collected here cover the whole range of Lashley's work with comparatively little overlap and preserve a nice balance between experiment and theory. Inevitably, every student of Lashley will have his pet omission (that of the present reviewer being a short article on mass action published in *Science* in 1931) but it will be widely agreed that the Editors could hardly have done better. There is a bibliography of Lashley's published work (which may well help to ensure that the papers excluded from this volume are not immediately forgotten) and two short appreciations by Professor E. G. Boring and Dr. Stanley Cobb respectively. It is a pity that a more personal account, such as that written by Sir Frederic Bartlett for the *Biographical Memoirs of Fellows of the Royal Society*, could not have been included.

O. L. ZANGWILL.

PUBLICATIONS RECEIVED

NEW JOURNALS

- Pavlov Journal of Higher Nervous Activity*. Vol. 9, No. 1, 1959. This is the first part of the *Pavlov Journal* to be published in translation by the Pergamon Press Ltd. The *Journal* is issued bi-monthly. Annual Subscription £10 10s.
- Journal of Child Psychology and Psychiatry and Allied Disciplines*. Vol. 1, No. 1, 1960. Joint Editors: C. B. Hindley, Elizabeth Irvine and Emanuel Miller. London. Pergamon Press Ltd. Subscription to Libraries and Institutions £7 per volume.
- Psicologia Industrial*. Vol. 1, Nos. 2 and 3, 1960. Argentina. Siam di Tella Ltda. Published Quarterly. English Summaries.

BOOKS AND OTHER PUBLICATIONS

Review in a later issue is not precluded by notice here

- The Nature of Stress Disorders*. Papers read at the Society for Psychosomatic Research held at the Royal College of Physicians in May, 1958. London. Hutchinson Medical Publications. 1959. Pp. xiii + 298. 25s.
- Mechanisation of Thought Processes*. National Physical Laboratory Symposium No. 10. London. Her Majesty's Stationery Office. 1959. Pp. x + 980. (2 Vols.) 50s.
- Pesquisa Nacional sobre o Nivel Mental da População Brasileira*. Reported by Pierre Weil (for the Comissão Nacional). Serviço Nacional de Aprendizagem Comercial-Departamento Nacional. 1959. Pp. 180.
- Psychiatric Dictionary* (3rd edition). By Leland E. Hinsie and Robert J. Campbell. London. Oxford University Press. 1960. Pp. ix + 788. 140s.
- Das Betteln der Zoo-Tiere*. By Karl H. Winkelsträter. (Beheft 39 der "Schweizerischen Zeitschrift für Psychologie"). Bern. Hans Huber. 1960. Pp. 92 + 16 illustrations. Fr/DM 14.80.
- Nerves and their Cure*. By C. Edward Barker. London. Allen & Unwin. 1960. Pp. 198. 16s.
- Persuasion: How Opinions and Attitudes are Changed*. By Herbert I. Abelson. London. Crosby Lockwood. 1960. Pp. ix + 118. 27s.
- L'Analyse du Destin: Le Moi Pontifex*. By Henri Niel. Bruges. Desclée de Brouwer. 1960. Pp. 185. 120 FrB.
- Polarité du Symbole*. Etudes Carmelitaines. Bruges. Desclée de Brouwer. 1960. Pp. 252. 150 FrB.
- Mision Psicopedagogica en la Patagonia*. By Osvaldo Jorge Ruda. Buenos Aires. Universidad del Sur, Instituto de Huminadedes. 1960. Pp. 137.
- The Neuropsychology of Lashley*. Selected papers of K. S. Lashley. Edited by F. A. Beach, D. O. Hebb, C. T. Morgan and H. W. Nissen. London. McGraw-Hill. 1960. Pp. xx + 564. 74s.
- Das Gestaltprinzip im Leben des Menschen und der Tiere*. By Karl Bühler. Enzyklopädie der Psychologie in Einzeldarstellungen. Volume 5. Bern. Hans Huber. 1960. Pp. 106. DM 22.
- Handbook of Research Methods in Child Development*. Edited by Paul Henry Mussen. London. John Wiley. 1960. Pp. x + 1061. 122s.
- Space and Sight*. By M. von Senden. Translated from the German by Peter Heath, with an appendix containing Essays by A. H. Riesen, G. J. Warnock and J. Z. Young. London. Methuen. 1960. Pp. 348. 42s.
- Quantitative Methods in Psychology*. By D. Lewis. London. McGraw-Hill. 1960. xii + 558. 74s.
- Art and Illusion*. By E. H. Gombrich. London. Phaidon Press. 1960. Pp. 466. 70s.
- Perspectives in Personality Research*. Edited by H. David and J. C. Brengelmann. London. Crosby Lockwood. 1960. Pp. xii + 370. 50s.
- Conflict, Arousal and Curiosity*. By D. E. Berlyne. London. McGraw-Hill. 1960. Pp. xii + 350. 58s.
- Studies in Mathematical Learning Theory*. Edited by Robert R. Bush and William K. Estes. London. Oxford University Press (for Stanford University Press). 1960. Pp. viii + 432. 92s.

- Mental Health Book Review Index*. Vol. 5, No. 2. (Whole No. 10.) July, 1960. Semi-annually compiled by the Editorial Committee and Contributing Librarians. Pp. 64. Subscription \$3 per year.
- Rorschachiana VII*. 4th International Rorschach Congress, Brussels, August, 1958. Schweizerische Zeitschrift für Psychologie und ihre Anwendungen, No. 40. Bern. Hans Huber. 1960. Pp. 187. Fr/DM 23.80.
- Psychotherapie für den Praktischen Arzt*. By Berthold Stokvis. Basle/New York. S. Karger A.G. 1960. Pp. x + 142. SFr. 14.50.
- Memory for Designs Test: Revised General Manual*. By Frances K. Graham and Barbara Kendall. Perceptual and Motor Skills Monograph Supplement 2—VII. 1960. Pp. 42. \$2.50.
- Psychology: A Study of a Science*. Edited by Sigmund Koch. Study I. Conceptual and Systematic. Vol. I. Sensory, Perceptual and Psychological Formulations. Pp. 704. 76s. Vol. II. General Systematic Formulations, Learning and Special Processes. Pp. 704. Vol. III. Formulations of the Person and the Social Context. Pp. 837. 97s. London. McGraw-Hill. 1959.
- Personality and Persuasibility*. By C. I. Hovland et al. London. Oxford University Press (for Yale University Press). 1960. Pp. xiv + 333. 40s.
- Abnormal Psychology*. By Walter J. Coville, Timothy W. Costello and Fabian L. Rouke. New York. Barnes & Noble Inc. (College Outline Series No. 94.) 1960. Pp. xv + 298. \$1.75.
- Handbook of Physiology. Section 1: Neurophysiology. Volumes 2 and 3*. Editor-in-Chief: John Field. Section Editor: H. W. Magoun. Executive Editor: Victor E. Hall. American Physiological Society (Sole Agents: The Williams and Wilkins Co., Baltimore). 1960. Vol. 2: pp. 781-1438; Vol. 3: pp. 1440-1968 + Subject Index Vols. 1-3, pp. 1969-2-13. Each volume \$20.
- Plans and the Structure of Behavior*. By George A. Miller, Eugene Galanter, and Karl H. Pribram. New York. Henry Holt. 1960. Pp. x + 226.

CORRESPONDENCE

Professor M. D. Vernon writes:

"I should like to take issue with your reviewer of Volume 11 of the *Annual Review of Psychology*. The articles in the Annual Review are valuable both to specialists and to semi- and non-specialists in listing papers which have escaped their notice; and in giving brief descriptions of the contents of these papers which indicate whether it is worth while referring to them and reading them in full. Personal bias inevitably operates if only certain papers are selected for inclusion, and work of considerable interest to some readers may be ignored and omitted. Furthermore, articles in the *Psychological Review* and *Psychological Bulletin* provide excellent selections and critical appraisals of which their authors think to be of special interest and importance."

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Part 2

IMPRINTING AND PERCEPTUAL LEARNING

BY

W. SLUCKIN* and E. A. SALZEN†

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Filial responses are first considered by reference to the initial stimulus situations. Findings on variability in responsiveness of chicks are reported and discussed. Facilitated responsiveness subsequent to visual stimulation is reported. The concept of critical period is examined in the light of other workers' and our own findings; it is concluded that imprinting ends as a result of its own action rather than through the effects of fear. Following responses are further considered by reference to the influences of early experiences and the act of following upon the occurrence and strength of subsequent responses. The degree of attachment to a moving object tends to be proportional to the amount of experience. Individual chicks were allowed to spend several hours following a box in a runway and their ability to discriminate between familiar and unfamiliar moving objects and static environments was studied. Strong evidence for imprintability to environment has been found. Imprinting phenomena are discussed in terms of perceptual learning.

INTRODUCTION

Following responses of newly-hatched nidifugous birds have been used by many workers to investigate imprinting, especially since Thorpe (1956) surveyed the subject and emphasized the importance of such studies. These responses have been employed in two ways. In one type of study, e.g. Fabricius (1951), Ramsay and Hess (1954), Jaynes (1956, 1958 *b*), Hess (1957), Guiton (1959), the birds were required to discriminate between objects by approaching and following one object rather than another. In the second type of study, e.g. Jaynes (1957, 1958 *a*), Moltz and Rosenblum (1958 *a*), the birds were required to respond repeatedly to a single object, and the degree of attachment to that object, i.e. imprinting, was presumed to be indicated by the occurrence, latency and strength of the following response. As originally stated by Lorenz (1937) imprinting involves the discrimination of species characteristics, which implies the production of different response patterns to different patterns of movement, form, colour and sound. It could be argued, therefore, that studies of the second type were not studies of imprinting but simply studies on the development and establishment of the approach and following responses, and that discrimination experiments were required to determine whether these responses had become attached to a specific object.

There is no doubt, however, that the occurrence and establishment of the following response is important in imprinting studies since this is the response required in the discrimination tests. Salzen and Sluckin (1959) attempted to investigate the incidence and duration of following responses in domestic chicks without reference to imprinting. The present paper contains observations and experiments on both the following response and imprinting.

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The first occurrence of the following response will be considered by reference to the stimulus situation in which it occurs, the differences in its incidence among individuals, and the differences in its incidence with age. Subsequent occurrences of the following response will be considered in terms of the effects of earlier experience of the object, and the effects of the act of earlier following. Finally, imprinting will be considered in terms of the stimulus situations which elicit following in experienced birds, and in terms of the extent to which such responses may be elicited by new stimulus situations.

I

THE ORIGINAL STIMULUS SITUATION

It is clear from the work of Fabricius and Boyd (1954) and Hinde, Thorpe and Vince (1956) that a wide variety of moving objects may elicit the following response in birds. Fabricius (1951) after a careful study with ducks concluded that the movements of parts of the body were of great importance in the release of the following response. Since then Smith (1960) has shown that movement in one plane in the form of a distant rotating black-and-white disc is an adequate stimulus to cause chicks to approach and give contentment calls. Further, both James (1959) and Smith have shown that a flashing light will also serve as a stimulus for this response. Collias (1952) has reported that certain repetitive sounds with low-frequency components will evoke the approach response. Fabricius (1951) and Weidmann (1958) have observed that short repeated calls will produce following in ducks. We have found that knocking, tapping, rustling of paper and clucking will make some chicks approach. It seems likely, therefore, that a wide variety of moderately intense visual or auditory disturbances of the environment that are brief and repetitive in character are capable of eliciting filial responses in chicks and ducklings.

In an earlier paper (Salzen and Sluckin, 1959) we reported, among other things, upon the incidence in domestic fowl of the following response to a moving box. We did not in that paper concern ourselves with the responsiveness of chicks to sound. However, in the course of our work we had collected data on the chicks' responsiveness to sound and on its relationship to the chicks' previous visual experiences. The method used in our experiments is described in detail in the earlier paper mentioned above. Very briefly, the behaviour of 450 chicks of Rhode Island Red flock was observed individually from 18 to 30 hr. after hatching: the following responses of each chick to a red box 8 in. $\times$ 6 in. $\times$ 6 in. high, suspended 2 to 4 in. above the floor, were tested in an alley 10 ft. $\times$ 2 ft. 6 in. $\times$ 2 ft. high. Each chick was so placed at the start that it could either run away from the box or follow it when the box was moved with a jerk away from the chick. The box was moved every 10 sec.; a responsive chick would move up to the box, and then the box movement would be repeated; when a chick did not follow, the box was returned to its earlier position. Chicks in one group were exposed individually to 50 movements of the box, in another group to 10 movements, and in a third group to no movements, spending 1 min. beside the stationary box. Our earlier paper is concerned with the responses of these birds to the movements of the box on later occasions. Here we report on the chicks' responsiveness to sound which was tested in each case by tapping on the side of the runway immediately after the chick had experienced 50, 10 or no movements of the box. Chicks tended either to ignore the noise or respond to it by approaching its source. Those that appeared to attend to the noise but failed to run to it were regarded as not responding.

Table I shows that responsiveness to this second stimulus was related to previous experience of box movement. The results as set out indicate that experience of the

moving box tended to facilitate responsiveness to the second stimulus complex. Smith, in a personal communication, has also reported a facilitation effect in that a small-diameter patch of flashing light evoked more approach responses from chicks that had previously experienced a large circle of flashing light than from those that had not had such an experience. In view of this facilitation effect, measures of the initial responsiveness of chicks to a moving object may vary considerably with their visual experience.

TABLE I

PART A

Number of movements of the box seen by the chicks in the visual test ..	0	10	50
Proportions of chicks that responded to sound after the visual test	$\frac{37}{150}$ (25%)	$\frac{58}{150}$ (39%)	$\frac{76}{150}$ (51%)
Probabilities of the indicated differences being chance occurrences, as determined by the Brown (1956) test	$\longleftrightarrow p < 0.05$		$\longleftrightarrow p < 0.05$
	$\longleftrightarrow p < 0.01$		

PART B

Number of movements of the box seen by the chicks in the visual test	Proportions of chicks that responded to sound among those that in the visual test had:—		Probabilities of the differences between columns being due to chance occurrences as determined by the Brown test
	followed the box at least once	not followed the box	
10	$\frac{45}{86}$ (52%)	$\frac{13}{64}$ (20%)	$p < 0.01$
50	$\frac{73}{120}$ (61%)	$\frac{3}{30}$ (10%)	$p < 0.01$

The table shows the results of sound tests carried out on day-old chicks immediately after they had been exposed to the moving-box test.

Part A of the table shows that greater experience of the moving box resulted in a greater proportion of the birds responding to sound.

Part B of the table shows that birds that had followed the box were more responsive to sound than were those that had not previously followed the box.

II

VARIABILITY IN RESPONSIVENESS

There is some evidence of constitutional differences in sensitivity to a moving object even within one breed of domestic chicks. Hess (1957) has claimed that imprintability, which may be related to this responsiveness, is inherited in domestic fowl and can be bred into or out of a strain, and that it is probably sex-linked in Cochin Bantams. Klopfer (1956) has also suggested a genetic explanation of reported differences in sensitive periods and imprintability. Table II shows inter-batch

TABLE II

<i>Source and nature of batches of chicks</i>	<i>Numbers of chicks tested for 3 min. with a moving box</i>	<i>Numbers of chicks that responded by following the box</i>	<i>Percentages of followers in the different batches</i>
<i>1st experiment (1959)</i>			
1st supplier:			
R.I.R. laboratory hatch ..	159	73	46
R.I.R. commercial hatch ..	96	49	51
<i>2nd experiment</i>			
2nd supplier:			
R.I.R. laboratory hatch ..	88	24	27.5
B.L. × L.S. laboratory hatch	103	19	18.5

The table shows the incidence of following in different batches of chicks during a test with a moving box, as described in the text, lasting 3 min. The differences between either hatch of the first experiment and either hatch of the second experiment are significant beyond the 1 per cent. level of confidence according to the Brown (1956) test.

variations in responsiveness encountered by us. Guiton (1959) found that visually isolated chicks followed better than socially reared ones, and that a few hours of social experience preceding a period of isolation resulted in greater responsiveness to a moving object than did complete isolation. In an experiment summarized in Table III we gave three pre-test treatments to Brown Leghorn × Light Sussex chicks. Our results tend, on the whole, to confirm Guiton's findings.

TABLE III

<i>Pre-test treatment of batches of chicks</i>	<i>Tested when 1 day old</i>			<i>Tested when 5 or 6 days old</i>
	<i>Batch A</i>	<i>Batch B</i>	<i>Batch C</i>	<i>Batch D</i>
Isolation (each chick was kept in a separate cage and could see no moving objects between hatching and test) ..	$\frac{4}{10}$ (40%)	$\frac{10}{24}$ (42%)	$\frac{20}{41}$ (49%)	$\frac{5}{15}$ (33%)
Social (groups of chicks were kept in enclosed cages between hatching and test) ..	$\frac{8}{14}$ (57%)	$\frac{8}{24}$ (33%)	$\frac{6}{41}$ (15%)	$\frac{0}{13}$ (0%)
Social for the first 6 hr. and then isolation	—	—	$\frac{28}{41}$ (68%)	—

The table shows the effects of social life on the responsiveness of chicks. The entries in the table are the proportions of birds that followed a moving box in the course of a 3-min. test. The proportions of isolated and social birds that responded were not at the 0.01 level of confidence using the Brown test and in Batch D at the 0.05 level using the Fisher test. In Batch C 6 hr. social life followed by 18 hr. of isolation resulted in a greater proportion of followers than did complete isolation (although the difference in the proportions was not quite statistically significant at the 0.05 level).

The influence of some social experience upon later responsiveness may account for the apparent rise in "imprintability" recorded by Jaynes (1957) for the first half of the critical period, since his birds had been kept in groups before testing. The same may apply to Hess's similar results with Mallards (1957) and chicks (1959 *a*), since although they were reared in darkness they could hear one another, and sound was used in both the imprinting and the testing situations; it should be noted that although the sounds on the two occasions were different, Guiton's generalization-through-isolation theory would predict that the difference would not prevent the test sounds from being effective. This explanation is not unlikely in view of the facilitation effect described in the previous section of this paper.

III

LOSS OF SENSITIVITY—THE CRITICAL PERIOD

The work of Fabricius (1951), Ramsay and Hess (1954), Weidmann (1956), Hess (1957) and Jaynes (1957) has shown that ducklings and chicks will not follow a moving object if exposure to that object is delayed for some 2 or 3 days. This early period has been called the "sensitive" or "critical" period and in general it is clear that ducklings and chicks are most easily imprinted during the first day or so after hatching. In our 1959 paper we noted that in individual cases chicks remained responsive for as long as 5 days. Guiton (1958) has shown that isolated chicks remain responsive to a moving object after 3 days by which time most socially reared chicks failed to respond. In a subsequent study Guiton (1959) concluded that socially reared chicks become imprinted to one another and that when subsequently exposed to a moving object they show "searching" behaviour and "distress" rather than strong and fearful avoidance of the moving object itself. Table III shows our own results for the responsiveness of socially reared chicks and isolated chicks to 3 min. exposure to a moving box under our standard testing conditions as described in our 1959 paper. It can be seen that in one out of three experiments 24 hr. social life was sufficient experience to significantly reduce responsiveness compared with isolates, and that in a fourth experiment only isolated birds remained responsive after 5 days. On another occasion three birds that had been isolated for 6 days responded vigorously within 3 min. of exposure to the moving box. There is support, therefore, for Guiton's contention that the end of the critical period results from the increasing selectivity of responsiveness with experience or, in other words, that imprinting brings about its own end.

An earlier explanation that the loss of responsiveness was due to the development of fear was first proposed by Hinde, Thorpe and Vince (1956) and it was supported by Hess (1957) on the basis of drug experiments. More recently Hess (1959 *a*) has claimed that all his chicks showed fear, i.e. distress notes and avoidance by 33 to 36 hr. and that this corresponds with the loss of the following response. Jaynes (1957) has recorded that weak flight begins to appear at 30 to 36 hr. and strong flight begins to appear at 54 to 60 hr. and he suggests that fear of strange moving objects in chicks reared socially may rise to a maximum in the first week of life and decrease thereafter. Guiton (1959) has stated that fear is developed sufficiently to interfere with following at 4 days whereas most socially reared birds are imprinted by 3 days. It is possible, therefore, that fear responses at least in the form of avoidance, are the responses that imprinted birds make to strange objects, i.e. to objects to which they are not imprinted.

Fabricius (1951), Weidmann (1958), Jaynes (1957), Guiton (1959) and no doubt other workers, too, have all observed that birds may show fear and avoidance of a

moving box at first, yet may subsequently come to follow the box most strongly. We have observed this. On three occasions we selected a chick that showed extreme avoidance of a moving box, isolated it, and exposed it to the moving box for 3 min. every day. These chicks finally began to follow and in this way we have had a chick responding for the first time when over 2 weeks old. On another occasion a similar procedure was used with three chicks that had remained "frozen" and three that had shown active avoidance of the box selected from a batch of chicks first tested when 1 day old. The former three followed well after a few days but the latter three had not begun to follow when the experiment ended after 12 days. It is possible that further treatment would have induced these birds to follow as in the previous experiment. Similarly we have isolated chicks for 2 or 3 days before exposing them to a moving box. At first some of these birds have shown avoidance and fear of the box but in all (six) cases prolonged exposure has resulted in following usually within the first hour. Jaynes (1957) has reported a similar observation with chicks 5 and 6 days old that had been reared socially, and Fabricius (1951) earlier recorded a similar experience with ducks but attributed it to feeding conditioning rather than to imprinting.

It seems, then, that habituation to the strange object may cause the avoidance response to disappear and the following response, and presumably imprinting, to occur. Hess (1959 *b*) has claimed that birds that are induced to follow objects in this way do not in fact become imprinted to these objects. Following similarly takes place under the influence of a tranquillizing drug, chlorpromazine (but not meprobamate) (Hess, 1957, Table I, item 4), and in this particular instance Hess seems to consider that imprinting did occur after the end of the critical period as judged by the performance of his control group.

Guiton (1959) has reported that although avoidance is most marked in isolated birds, experience of the moving object usually reduces fear and results in following. In socially reared birds avoidance or crouching also disappears due to habituation but in most cases this does not result in following. Instead these birds show "searching" behaviour with distress calls and the model is ignored, i.e. true habituation to the model has taken place. We feel therefore that the evidence is against fear being the cause of the end of the sensitive period, and that imprinting can end only as a result of its own action in increasing the selectivity of the animal's responses.

IV

THE EFFECT OF EXPERIENCE

In the work considered here the following response has been used as an indicator of the development of filial responsiveness. This development is shown both by the bird's immediate response to experience of a moving object and by its performance after a time interval, i.e. by the amount of retention of responsiveness. In a study of this kind Jaynes (1958 *a*) has shown that under his particular experimental conditions 10 min. experience of a moving box was sufficient to induce following in some day-old chicks, but that 40 min. experience was required before all his chicks responded. Furthermore this experience was effective whether given on the first day or spread over the first 4 days and the greater the amount of experience the greater was the probability of a response 30 and 70 days later. Jaynes used six chicks for each condition in his experiment. In a similar study (1959) we found that the percentage of chicks that followed a moving box on the first day after hatching was approximately proportional to the amount of experience of that box, so that after 10 movements of the box 50 per cent. of the chicks had begun to follow and after

50 movements 80 per cent. were following. Under test conditions similar though not identical to those of Jaynes we could detect a significant difference between the chicks that had received these two amounts of experience which approximated to 2 and 8 min. exposure. We also recorded a considerable variability in the responsiveness of chicks within and between batches and therefore felt that further data were desirable.

In Table IV are the results of an experiment carried out by us in which chicks from a Brown Leghorn $\times$ Light Sussex cross were subjected to one or other of three treatments. Using the experimental procedure described in our previous paper, chicks were given 3 min. or 30 min. experience of a moving box at 1 day of age, or

TABLE IV

<i>Amount of experience of a moving box</i>	<i>3 min.</i>	<i>30 min.</i>	
Distribution of this experience ..	One session of 3 min. on the first day	Three sessions of 10 min. on the first day	One session of 6 min. on each of the first 5 days
Proportions of birds that responded to the moving box in a 3-min. test on sixth day after hatching	$\frac{3}{16}$ (19%)	$\frac{6}{14}$ (43%)	$\frac{6}{11}$ (55%)

The table shows the effects in socially reared chicks of the amount and distribution of experience of a moving box on subsequent responsiveness to this stimulus. The probability that the difference between the first and third columns was a chance occurrence is $p = 0.032$, as determined by the Fisher exact probability test.

6 min. experience on each of the first 5 days after hatching. The only difference from our previous procedure was that chicks that failed to respond within the first 3 min. were rejected. All the chicks were tested for 3 min. in the same situation on the sixth day. The results in Table IV confirm Jaynes' claim that within these conditions the degree of imprinting is proportional to the amount of experience of the moving object. Further the fact that we were able in our previous experiment to detect the effect of 10 movements of a box on subsequent responsiveness of chicks suggests that the mechanism is extremely sensitive but requires strengthening by repetition.

V

THE EFFECT OF EFFORT

The experiments described in the last section did not separate the effects of visual experience of the moving object from the effects of the act of following. Hess (1957) attempted to do this by running ducks at different speeds for the same length of time, and he found that the longer distance resulted in better imprinting as judged by responsiveness in discrimination tests. It could be, however, that the faster-moving object is a more potent stimulus than the slower-moving one. In our own work (1959) we were unable to find any relation between the distance run by chicks on first exposure to a moving box and their performances on subsequent occasions. Hess's (1957) results with drugs appeared to lend support to his assertion and he suggested that meprobamate, being a muscle relaxant, prevented imprinting by interfering with the "muscular tension or other afferent consequences" of following. Yet at the same

time he stated that this drug did not interfere with motility or co-ordination either in the training or test sessions. It is difficult to see how these two claims can be reconciled. In the same experiment chlorpromazine gave good imprinting under all conditions, yet Ryall (1958) and Taeschler and Carletti (1959) have reported that chlorpromazine simultaneously reduces emotional behaviour and motor activity in rats and mice. Hess (1958) has found that obstructions in the runway resulted in stronger imprinting because of the greater effort required in following, and this is perhaps the strongest evidence for the effect of effort. In a more recent paper Hess (1959 *a*) showed for chicks that the ability to run improved over the first 16 hr. during which time "imprintability" showed a corresponding improvement. A correlation of this kind does not prove the causal relationship between effort and imprinting. In any case chicks that cannot run may make considerable efforts to follow, and so the relationship must be between running and imprinting, in which case the result of the obstruction experiment remains unexplained.

Jaynes (1957) has also found that although the percentage of chicks responding decreased throughout the critical period the strength of imprinting judged by retention tests tended to increase during the first day, i.e. as locomotory ability increased. Poor locomotory ability does not necessarily mean poor locomotory effort, but in both Jaynes' and Hess' experimental circumstances it may well result in the chick spending very little of the training time near to the box and as a result obtaining very little experience of the box. In our own experimental circumstances (1959), where the box was never more than 12 to 18 in. from the chicks, we found that very good runners often failed to respond in the later tests whereas some non-runners subsequently performed very well. Unlike Hess' chicks, however, all our birds had social experience between the tests and this may have affected their responsiveness in the second test. In a recent study Baer and Gray (1960) allowed chicks to see but not to follow or contact a white or a black guinea-pig during one of the first 4 days after hatching. At 7 days the chicks were tested in a choice situation with black and white guinea-pigs. They spent significantly more time near the familiar than near the unfamiliar guinea-pig, and this effect was strongest when the previous experience of the guinea-pig had been on the second day after hatching. It would seem therefore that there is evidence that some imprinting can occur without the act of following, and that the evidence concerning the role of the effort of following is not yet convincing.

VI

LATER STIMULUS SITUATIONS

As imprinting progresses so the bird becomes less likely to respond to stimuli other than the experienced one or ones. The extent to which a bird will generalize is, therefore, an inverse measure of the degree of imprinting. The first effect of experience appears to be one of facilitation of responsiveness to stimulation, as has been described earlier. Further experience restricts this responsiveness to an increasingly well defined stimulus pattern, as has been suggested by the individual recognition of two humans by chicks reported by Gray and Howard (1957). In Guiton's (1959) study, after 2 or 3 days of social life chicks had become imprinted specifically to chicks in that they would not follow other objects which they could thus discriminate from chicks.

Repeated presentation of the imprinted stimulus may lead to an apparent loss of the following response. Thus, Moltz and Rosenblum (1958 *a*) have reported that with repeated daily experiences of a moving box the following response of Peking

ducks wanes. We observed that when chicks were exposed singly to an intermittently moving box (in a mechanized version of our standard imprinting runway with a suspended white cylindrical box which travelled up and down the centre of the 10-ft. alley in alternating $\frac{1}{2}$ -min. periods of rest and movement), they all began to follow within the first hour and stopped following after several hours. A warm hover with food and water was situated to one side of the alley and the chicks spent progressively more time in their vicinity. In the experiment of Moltz and Rosenblum it could be said that the birds had become habituated to the stimulus situation, and in our experiment they had become at least temporarily satiated with the stimulus. However, the box had not ceased to be a stimulus to the chicks, for very strong and close following could be produced simply by the experimenter appearing and waving his arms, shouting, clapping and stamping his feet. When then left undisturbed the chick would gradually cease following the box once more.

This last observation is particularly interesting in view of Hess's (1959 *a*) suggestion that fearfulness stops the following response from appearing. Moltz and Rosenblum (1958 *b*) have shown that if ducks are first habituated to the testing enclosure, then they are less responsive to a moving box than birds not so treated.

Moltz *et al.* (1959) have shown that electric shock treatment given inside the testing enclosure also increased the level of responsiveness of ducks. The differences in responsiveness of birds treated in these ways have been ascribed by these workers to the effect of anxiety.

In further observations on six chicks that had become "satiated" with and had ceased to follow the intermittently moving box as previously described, we found that following may be restored in two ways. Firstly, by introducing a second moving object (a red duster) and secondly by quietly and surreptitiously introducing strange static objects into the alley. In each case, as soon as the chick noticed the strange objects it began to follow the moving box with "anxious" chirps. These observations provide a possible solution to the apparent contradiction between the effects of "anxiety" in facilitating following and "fearfulness" in inhibiting following. If fear is taken to be the avoidance of a stimulus situation, then there may be avoidance of a fluctuating stimulus pattern (figure) and avoidance of the environment, or stable stimulus situation from which the figure is differentiated (ground). Filial responses are to the figure, and fear of a figure thus prohibits such responses. We have suggested in an earlier section that fear is simply the response that birds make to objects to which they are not imprinted. Thus the imprinted bird not only goes to the figure to which it has become attached, but also avoids the strange figure. The establishment of the attachment of a bird to its environment is not so easily revealed in the way that attachment to a figure is revealed by the following response. In our experiment, however, the relationship was revealed when the satiated birds avoided the strange features of the environment by resuming their approach and following responses to the familiar moving box. Thus it is clear that filial responses can be restored by making some change in the environment and that birds come to discriminate by approach and avoidance responses both between familiar and unfamiliar moving objects and between familiar and strange environments. It is not clear whether both processes are imprinting, but Thorpe (1956) has already suggested that an exclusive attachment to an environment may be a form of imprinting.

If changes in the environment or ground will restore responsiveness to the imprinted moving object or figure it is possible that there is also some loss of discrimination, i.e. increased responsiveness is accompanied by increased generalization. This might account for the response of social chicks to a moving object after they have been isolated for a time. We have tried taking a chick that had been with the

white cylinder for several hours out of the runway, substituting a red duster for the cylinder and immediately replacing the chick. If "anxiety" caused by this procedure resulted in generalization (and this treatment certainly causes chicks that have ceased following to resume following the familiar box) then such a chick should have followed the duster. We found that in all of four cases the chick showed avoidance of the duster. We feel, therefore, that anxiety effects cannot explain the generalization phenomena found by Guiton (1959) and that some process of forgetting must be involved. The anxiety effect, however, may explain why Hinde, Thorpe and Vince (1956) could not get any waning of responses to the figure in coots, since at the end of each run the birds were chased, caught and handled. Yet these workers found that responses to moderately strange objects waned gradually, suggesting that the discriminatory ability asserted itself over anxiety. These same workers tried the familiar objects in strange environments with mixed results: moorhens did not follow and coots followed less well. Clearly, further research is needed in this field.

VII

REVERSIBILITY

Thorpe (1956) has already quoted several observations that suggest that imprinting is not irreversible. It is apparent from a previous section that attachments can be induced after the end of the so-called critical period provided that the bird has not been previously exposed to moving objects. Guiton (1959) has shown that socially reared birds become imprinted to one another in that they will not respond to a moving box. If such birds are isolated for a time they may subsequently respond to other objects. Table V shows our own results for experiments of this kind. It can be seen that day-old socially reared chicks did not respond to a moving box until they had been isolated for 3 hr. In another experiment chicks reared socially for 2 days would not respond after 3 hr. isolation but would do so after 24 hr. isolation. If, as has been suggested in an earlier section of this paper, the strength of social attachment is proportional to the amount of experience, then it might be expected that the

TABLE V

<i>Batch of chicks</i>	<i>Pre-test treatment of newly-hatched chicks</i>	<i>Proportions of birds that responded to the 3-min. experience of a moving box by following</i>
E	21 hr. of social life followed by 3 hr. of isolation	$\frac{7}{8}$
	24 hr. of social life	$\frac{0}{6}$ Probability of this difference being a chance occurrence is $p = 0.0023$
F	48 hr. of social life followed by 3 hr. of isolation	$\frac{0}{8}$
	48 hr. of social life followed by 24 hr. of isolation	$\frac{2}{3}$ Probability of this difference being a chance occurrence is $p = 0.054$

The table shows the effect of a period of isolation on the responsiveness of socially reared chicks to 3 min. experience of a moving box. It also indicates that the length of the isolation period necessary to produce this effect may be related to the amount of social experience of the chicks. The probabilities recorded in the table were determined by the Fisher exact probability test.

reversibility could be similarly affected so that the longer the social experience the longer the isolation required before responses to strange objects can be obtained. In this case the removal of the familiar objects and habituation to new objects might well produce the kinds of behaviour reported by Goodwin (1948), Steven (1955) and perhaps even the case of the Muscovy Ducks reported by Lorenz (1937). Goodwin's birds responded sexually to the species with whom they were reared and not to their parent species with whom they had spent only the first week or so of their lives. Lorenz's ducklings were reared for nearly 7 weeks with Greylag geese before being removed, and it is therefore not impossible that one of them should subsequently still prefer geese. It may be that the reversibility of imprinting involves the acquisition of new objects but not necessarily the loss of old ones. There is evidence in the reports of Fabricius (1951), Hinde, Thorpe and Vince (1956), Jaynes (1957) and Guiton (1959) that birds may become attached to several objects. In view of the relation between amount of experience and strength of attachment one might expect that the birds' preferences for objects would be related to the relative amounts of previous experience of these objects. In this respect, therefore, Lorenz' observation is still a remarkable one.

VIII

IMPRINTING AND PERCEPTUAL LEARNING

As implied in the introduction, it is probably best to restrict the term "imprinting" to those experimental situations in which the "imprinted" bird is required to discriminate between patterns of movement, form, colour, sound, etc., and not simply to repeat earlier responses to the same stimulus pattern or object. The development of the imprinting process, or attachment to a class of stimuli, consist of a sharpening of discrimination between this and other classes. It would appear that this view of imprinting conforms to the criterion of perceptual learning put forward by Wohlwill (1958). As perceptual learning takes place, "each stimulus to be discriminated must be classifiable, *qua* physical stimulus, into the class of stimuli to which the corresponding differential response is preattached." As Wohlwill points out, "paired-associate learning and similar tasks are left outside the province of perceptual learning situations."

The development of attachment to environmental stimuli, or imprinting, seems to fit in with the progress of perceptual learning, as outlined by Gibson (1959). He has suggested that a distinction may be made between the environment and the social components of the environment in terms of the nature of the physical stimulation which they provide. Briefly, in the case of vision, the environment is one of surfaces, whereas the social or animal environment is one of movement and deformation of surfaces. Gibson contends that these are the kinds of information that the organism receives and has to differentiate. We suggest therefore that the process of which imprinting is a part may be considered in these two divisions, i.e. perception of the inanimate environment and perception of the animate environment, and that the development of social responses is part of the second of these two categories. In the course of the development of their social responses the chicks have first to discriminate the moving surfaces from the static. The progress of discrimination may be revealed by the chicks' contentment calls and their approach and following responses. As the chicks' perceptions of the stimulus situation become further differentiated with respect to movement, form, colour and sound, so discrimination in the form of choice of object-following will occur and this is the process of imprinting as originally conceived.

If "imprinting" is extended to cover the development of the tendency to orientate social responses to moving or changing stimulus patterns, such as a flashing light, a source of intermittent sound, or a moving object, then the concept becomes identical with that of the development of perception of the animate or social environment. If "imprinting" is also to cover the development of perception of the inanimate environment, then the discussion by Thorpe (1956) of cases of attachment to environments and our own observations of the effect of changes in the static environment on the chick's following response become relevant. The imprinting process then, could be regarded as part of a process of developing perception resulting from repeated stimulation. Gibson (1959) in considering this type of phenomenon, has asked, "Do the qualities of a thing emerge from repeated impressions of the same thing or only by contrast with different things?" He considers that existing evidence on the perception of unfamiliar objects in laboratory situations favours the latter, but he points out that the former possibility has not been studied. Some of our studies on the following response are, however, a step in this direction. "Repeated impressions of the same thing" occur when a chick is exposed for a time to a moving object. In one of our experiments we exposed chicks individually for a few hours to a moving white cylinder. These chicks then discriminated between the moving white cylinder and a similarly moving red duster, either when the two were presented together or when they were presented consecutively. It would seem, therefore, that in imprinting the view that the qualities of an object may emerge simply from continued impressions of it must be seriously considered.

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REFERENCES

- BAER, D. M., and GRAY, P. H. (1960). Imprinting to a different species without overt following. *Percept. mot. Skills*, **10**, 171-4.
- BROWN, J. (1956). A modified significance test for the difference between two observed proportions. *Occup. Psychol.*, **30**, 169-74.
- COLLIAS, N. E. (1952). The development of social behaviour in birds. *Auk*, **69**, 127-59.
- FABRICIUS, E. (1951). Zur Ethologie junger Anatiden. *Acta zool fenn.*, **68**, 1-175.
- FABRICIUS, E., and BOYD, H. (1954). Experiments on the following reactions of ducklings. *Wildfowl Trust Annual Report, 1952-3*, 84-8.
- GIBSON, J. J. (1959). Perception as a function of stimulation. In *Psychology: A study of a science*. Study I, Vol. 1, 456-501. Edited by S. Koch. New York.
- GOODWIN, D. (1948). Some abnormal sexual fixations in birds. *Ibis*, **90**, 45-8.
- GRAY, P. H., and HOWARD, K. I. (1957). Specific recognition of humans in imprinted chicks. *Percept. mot. Skills*, **7**, 301-4.
- GUITON, P. (1958). The effect of isolation on the following response of brown Leghorn chicks. *Proc. R. phys. Soc. Edinb.*, **27**, 9-14.
- GUITON, P. (1959). Socialization and imprinting in brown Leghorn chicks. *Anim. Behav.*, **7**, 26-34.
- HESS, E. H. (1957). Effects of meprobamate on imprinting in water-fowl. *Ann. N.Y. Acad. Sci.*, **67**, 724-32.
- HESS, E. H. (1958). "Imprinting" in animals. *Sci. Amer.*, **198**, 81-90.
- HESS, E. H. (1959 a). Two conditions limiting critical age for imprinting. *J. comp. physiol. Psychol.*, **52**, 515-8.
- HESS, E. H. (1959 b). Imprinting. *Science*, **130**, 133-41.
- HINDE, R. A., THORPE, W. H., and VINCE, M. A. (1956). The following response of young coots and moorhens. *Behaviour*, **9**, 214-42.
- JAMES, H. (1959). Flicker: an unconditioned stimulus for imprinting. *Canad. J. Psychol.*, **13**, 59-67.
- JAYNES, J. (1956). Imprinting: the interaction of learned and innate behaviour. I. Development and generalization. *J. comp. physiol. Psychol.*, **49**, 201-6.

- JAYNES, J. (1957). Imprinting: the interaction of learned and innate behaviour. II. The critical period. *J. comp. physiol. Psychol.*, **50**, 6-10.
- JAYNES, J. (1958 a). Imprinting: the interaction of learned and innate behaviour. III. Practice effects on performance, retention and fear. *J. comp. physiol. Psychol.*, **51**, 234-7.
- JAYNES, J. (1958 b). Imprinting: the interaction of learned and innate behaviour. IV. Generalization and emergent discrimination. *J. comp. physiol. Psychol.*, **51**, 238-42.
- KLOPFER, P. (1956). Comments concerning the age at which imprinting occurs. *Wilson Bull.*, **68**, 320-1.
- LORENZ, K. Z. (1937). The companion in the bird's world. *Auk*, **54**, 245-73.
- MOLTZ, H., and ROSENBLUM, L. A. (1958 a). Imprinting and associative learning: The stability of the following response in Peking ducks (*Anas platyrhynchos*). *J. comp. physiol. Psychol.*, **51**, 580-3.
- MOLTZ, H., and ROSENBLUM, L. A. (1958 b). The relation between habituation and the stability of the following response. *J. comp. physiol. Psychol.*, **51**, 658-61.
- MOLTZ, H., ROSENBLUM, L. A., and HALIKAS, N. (1959). Imprinting and level of anxiety. *J. comp. physiol. Psychol.*, **52**, 240-4.
- RAMSAY, A. O., and HESS, E. H. (1954). A laboratory approach to the study of imprinting. *Wilson Bull.*, **66**, 196-206.
- RYALL, R. W. (1958). Effect of drugs on emotional-behaviour in rats. *Nature, Lond.* **182**, 1606-7.
- SALZEN, E. A., and SLUCKIN, W. (1959). The incidence of the following response and the duration of responsiveness in domestic fowl. *Anim. Behav.*, **7**, 172-9.
- SMITH, F. V. (1960). Towards a definition of the stimulus situation for the approach response of the domestic chick. *Anim. Behav.*, **8**, 197-200.
- STEVEN, D. M. (1955). Transference of "imprinting" in a wild gosling. *Brit. J. anim. Behav.*, **3**, 14-6.
- TAESCHLER, M., and CERLETTI, A. (1959). Differential analysis of the affects of phenothiazine-tranquillizers on emotional and motor behaviour in experimental animals. *Nature, Lond.*, **811**, 823.
- THORPE, W. H. (1956). *Learning and instinct in animals*. London.
- WEIDMANN, U. (1958). Verhaltenstudien an der Stockente (*Anas platyrhynchos*. L.). II. Versuche zur Auslösung und Prägung der Nachfolge und Anschlussreaktion. *Z. Tierpsychol.*, **15**, 277-300.
- WOHLWILL, J. F. (1958). The definition and analysis of perceptual learning. *Psychol. Rev.*, **65**, 283-95.

IMITATIVE RESPONSES AND THE RATE OF GAIN OF INFORMATION

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A series of experiments is described on audio-verbal reaction times. In the first experiment the signals were letters or digits recorded on magnetic tape and presented through earphones. In the second experiment the signals were nonsense syllables drawn from vocabularies of different size. The subjects responded by repeating what they heard as soon as it was presented and reaction times were recorded. It is shown that the size of the group from which the signal is drawn has little effect on the reaction time to that signal. A distinction is drawn between situations in which the rate of gain of information may be expected to apply and situations in which it may not apply. It is suggested that many highly practiced skills fall into the latter category.

INTRODUCTION

Since Hick (1952) first applied information theory to account for the way in which reaction time to a signal varies with the number of alternatives from which it is drawn, there have been a number of studies confirming the result that choice reaction time is directly proportional to the amount of information conveyed by the stimulus (cf. Bricker's review, 1955). The use of information measures promised to provide a means of comparing different experimental situations in terms of their formal properties, without reference to the actual signals or responses used.

Other experiments have shown, however, that the amount of information in the signal is not the only determinant of reaction time. Crossman (1953 and 1955) demonstrated the effect of different discriminability of signals within sets of the same size, and Fitts and Seeger (1953) emphasized the importance of "compatibility" between stimulus set and response set. More recently Mowbray and Rhoades (1959), using a light-key situation, showed that after really prolonged practice there was no difference between 2-choice and 4-choice reaction time. Leonard (1959) used a situation in which tactile stimulation was applied to the fingers and the required response was to withdraw the finger stimulated. In this situation no systematic differences were found among 2-, 4-, or 8-choice reaction times. Both of these experiments raise doubts about the general application of the rate of gain of information to disjunctive reaction times. Several authors have suggested that the rate of gain function should only apply when information is optimally coded. Only in this case can the full-channel capacity of the system be used. If information is not optimally coded then the amount of information transmitted per second is less than the channel capacity. However, if an attempt is made to explain the results of these experiments in terms of differences in coding, then in order to obtain similar reaction times for 2-, 4-, and 8-choice conditions, the set of 8 signals would have to be coded more efficiently than the set of 4, which in turn would have to be coded more efficiently than the set of 2, and these differences in efficiency of coding would have exactly to compensate the different amount of information in the stimulus sets. Of course, one can call this effect "coding" but we still have to explain the difference in efficiency.

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The notion of stimulus-response compatibility introduced by Fitts and Seeger (1953) was further developed by Deininger and Fitts (1955) and by Fitts (1959). Minimum reaction times should be expected in situations where S-R compatibility is a maximum and these authors propose that this maximum is approached when the following two conditions are met:

- (1) The stimulus set corresponds in a direct physical sense to the response set.
- (2) The pairings of stimulus and response elements agree with strong population stereotypes.

This compatibility effect is assumed to be related to "coding." In comparing disjunctive reactions with the same degree of choice, a more compatible S-R ensemble would be expected to produce shorter reaction times than a less compatible ensemble, on the basis of this effect, but nothing is implied about variations in reaction time with degree of choice within the same stimulus-response ensemble. Whether there is a difference in the way in which reaction time varies with degree of choice in high, compared with low, compatibility situations is a different issue, which was examined for some situations by Irby (1956).

It should be noticed that neither of the two conditions mentioned by Fitts and collaborators is used to *define* compatibility. In fact, Deininger and Fitts are careful to distinguish the way in which S-R compatibility is measured from the way in which a population stereotype is measured. "First, compatibility effects are measured under instructions which specify a 'correct' ensemble of stimulus response pairings, whereas the population stereotype is specified from data gathered in essentially a 'free response' situation. Second, population stereotypes are measured before any special training is given, whereas compatibility effects usually are measured at the end of a period of training. . . . The relative compatibility of any particular perceptual-motor task, is defined as the ratio of the actual rate of information transferred (computed from the actual performance) to the maximum rate for that type of task." This definition is subsequently modified by Fitts (1959) so that the denominator is the average rate, rather than the maximum rate, for that kind of task.

Now it is implicit in such a definition that the comparison of compatibility between different stimulus-response relationships be made for equal amounts of practice on the specific tasks being compared. This was made explicit by Leonard (1959). It follows that we must distinguish between better performance as a result of higher S-R compatibility and better performance as a result of practice in a specific situation. If we omit this qualification then it would follow that the group giving the better performance would *ipso facto* be operating under conditions of higher S-R compatibility.

To the present authors it seems more useful to keep this qualification, which implies defining S-R compatibility in such a way that the compatibility of an S-R relationship does not alter with extended practice in a specific situation. Thus, we should want to be able to make the statement "This is a highly incompatible situation for the general population. However, these particular subjects have been trained in it for so long that they can perform as well as in a more compatible situation (for which they have not had so much specific training)." Now if we leave out the qualification we should have to say, "Previously this was a highly incompatible situation (for the population at large). It is now a highly compatible situation (for this particular group of subjects)"—although it may still be a highly incompatible situation for the population at large. Of course, one could introduce two meanings of "compatibility" to cope with these situations, but in the present authors' opinion

it is more useful to keep "compatibility" to refer to the "naturalness" of a stimulus-response relationship in the population at large (as measured in one of the ways suggested by Fitts) and not to refer to the improvement brought about by learning (or overlearning) a particular S-R relationship by a particular group of subjects as an increase in compatibility.

A specific example taken from the experiments to be described in this paper might be useful. The response of repeating a word which the subject had just heard was used in this series of experiments. On *a priori* grounds this seems to be a situation of high stimulus-response compatibility, more compatible, for example, than responding with a different word. Now in such a situation one can obviously use words (or nonsense syllables) very unfamiliar to the subject, and the speed at which he responds might improve as he becomes more familiar with the words used. It seems inappropriate to ascribe this improvement to increased compatibility between stimulus and response. The authors would prefer to say that compatibility remained the same throughout, but that increased familiarity with the words lead to the improvement, reserving differences in compatibility to describe situations in which, for example, the response was to produce a different word, or the same word backwards.

These remarks amount to a recommendation about the way in which the term "compatibility" should be used, and others may prefer a different usage. However, to limit its use in the way the present authors describe seems to them to carry distinct advantages.

The experimental situation used by the present authors involved the subject repeating verbally, as quickly as possible, whatever signal he heard, and choice reaction time experiments were carried out in which the auditorily presented signals were drawn from ensembles of different sizes.

Two groups of experiments were conducted:

- (1) Auditory-verbal reaction times to digits 0-9 (10 choice) compared with letters A-Z (26 choice).
- (2) Auditory-verbal reaction times to nonsense syllables drawn from sets of different sizes.

The authors chose the imitative response of repeating back whatever was heard since it seemed to have a high degree of stimulus-response compatibility. It should perhaps be mentioned that no operational procedure was carried out to establish that this was a highly compatible situation. Since the exact degree of compatibility is not an issue in the present experiments, this does not seem a vital objection; there are certain stimulus-response relationships which obviously have a higher degree of compatibility than others. Thus, although Leonard (1959) points out that "compatibility" is operationally defined, he does not carry out any series of operations to establish that responding with the finger stimulated is more compatible than, say, responding with a different finger. In both Leonard's case, and in the present authors' case, the operational procedures seem unnecessary, the "naturalness" of the relationship is so evident, although, of course, if one wants to go on to make inferences based on different degrees of compatibility, then clearly the operational procedures should be carried out.

EXPERIMENT I. LETTERS AND NUMBERS

Method

Eight lists of 26 letters were compiled by making eight different random-order arrangements of the alphabet. Eight different lists of 26 digits, 0-9, were also compiled by taking digits 0-9 twice, adding 6 digits randomly selected, and randomizing the order over the whole 26. These lists were then recorded on different magnetic tapes by one of the

experimenters (A. T.), allowing an interval of 10 sec. between each item in a list. Two practice letters were added before each list of 26 letters and 2 practice numbers before each list of 26 numbers. "o" was pronounced "nought."

The signals were presented binaurally to the subject, through earphones at a comfortable intensity (about 70 dB.). His instructions were to repeat verbally as quickly as he could, without error, each number or letter he heard. He was always informed of whether to expect a list of numbers or of letters.

The signal from the tape closed a circuit which started a set of dekatron counters. The subject wore a throat microphone connected to a voice key and his verbal response was used to stop the counters. Thus a measure of auditory-verbal reaction time to the nearest millisecond could be obtained.

The experiment was carried out using the other two authors, N.M. and R.D., as subjects. It should be emphasized that they had nothing to do with the preparation of the actual lists and could not have been familiar with them. Over two months later, after a summer vacation, the author A. T. acted as a subject, using the lists she had herself prepared. The fact that her results are so similar to those of the other two subjects suggests that even if she remembered anything from the preparation of the lists, it was not carried over to the reaction time situation. Her results have therefore been included, although the results of each subject are considered separately.

The experimental sessions were preceded by one practice session, consisting of one extra list of numbers and one extra list of letters. The eight experimental sessions then followed, each consisting of one list of numbers and one list of letters, with the order alternated in successive sessions.

Results

In Table I are presented Means and S.E.s of the reaction times for the three subjects over the eight sessions. The means for the letters are consistently slightly larger, but the difference between letters and numbers is very small.

TABLE I
EXPERIMENT I
REACTION TIMES FOR REPEATING NUMBERS/LETTERS FOR THE
THREE SUBJECTS OVER EIGHT SESSIONS

Stimulus list	Subject N. M.				Subject A. T.				Subject R. D.			
	Numbers		Letters		Numbers		Letters		Numbers		Letters	
	Mean	S.E.	Mean	S.E.	Mean	S.E.	Mean	S.E.	Mean	S.E.	Mean	S.E.
1	218	12.0	253	8.0	185	9.6	223	8.6	198	8.1	240	9.7
2	236	11.5	214	8.5	218	11.7	219	10.4	213	10.9	241	9.7
3	206	7.2	208	9.5	202	9.5	206	7.0	207	10.9	247	8.3
4	195	8.9	205	9.0	197	8.4	218	10.0	211	8.9	252	12.0
5	192	8.7	206	8.1	183	8.0	214	9.5	192	7.6	233	8.5
6	188	8.1	203	9.2	186	10.0	220	11.0	202	9.9	213	9.9
7	202	8.0	210	10.0	199	9.8	213	9.8	194	9.6	238	9.3
8	192	9.0	215	11.1	181	7.6	221	7.6	190	9.0	220	9.9

Means and standard errors in millisecond. N is approximately 26 for each entry. Slight variations in N were caused by an occasional omission or error, on the part of the subject, but these were very few.

Before evaluating this difference some consequences of the experimental procedure should be examined.

(1) In preparing the lists of letters, sampling without replacement was used, in order to equalize the frequency of each letter within the same list and to ensure the same amount of practice on each letter at each session in the experiment. This has the consequence that towards the end of each list there is more constraint on which

letter will appear next than there is at the beginning of the list. This is true to a lesser degree for the numbers. If this difference were utilized by subjects it could possibly produce relatively faster reaction times towards the end of each list of letters, and hence reduce any difference between the times for letters and numbers. In order to examine whether this happened, the mean RTs for the first ten items in each list were compared with the mean RTs for the last ten items in each list, taken over all the eight lists. These values are shown in Table II. If there is an effect resulting from the greater constraint on the selection of letters toward the end of each list, then the difference between the means for the first ten items and the last ten items should be greater for the letters than for the numbers. No consistent effect of this sort is shown. The differences are insignificant, and not always in the same direction.

TABLE II
EXPERIMENT I

OVERALL MEAN REACTION TIMES IN MILLISEC. OVER EIGHT SESSIONS FOR FIRST TEN ITEMS IN EACH STIMULUS LIST COMPARED WITH LAST TEN ITEMS IN EACH LIST

	Subject N. M.		Subject A. T.		Subject R. D.	
	Numbers	Letters	Numbers	Letters	Numbers	Letters
First ten items	195	207	191	219	200	234
Last ten items	208	217	194	212	197	230

(2) In any comparison involving different ensemble size, one cannot balance practice equally over the ensembles, and at the same time have equal practice on the separate items. Under the condition of the present experiment there was over twice as much practice on any individual number as compared with any individual letter during the course of any one session. This was an inevitable consequence of the experimental design, but it should be noticed that the greater practice on individual numbers should tend, if anything, to increase any difference in reaction time between numbers and letters.

(3) When using an auditory method of presentation there is some uncertainty about which point should be taken as the onset of the signal. There is a similar uncertainty about the onset of the verbal response. Thus, the subject may be hearing the letter/number before the key responds. Hence, the reaction time on the counters would be too small. On the other hand, the key may not operate at the beginning of the subject's verbal response. Hence, there may be a delay in stopping the counter and the apparent reaction time would be too large.

Control Experiments

(A) A series of "pips" from an oscillator were recorded on magnetic tape at intervals of approximately 10 sec. The "pips" were presented as a stimulus series and the subject was asked to respond by going through the alphabet, saying "A" to the first pip, "B" to the second, and so on. The variability in the time at which the signal from the tape starts the counter is now reduced. The response can be prepared in advance, so there is no uncertainty about the response at the moment the signal is given. If there is a constant difference in reaction time when saying one letter compared with another, this must be the result of variability in the time at which different verbal responses stop the counter. Hence, if the mean RTs for repeating different letters are taken, this gives a control measure for the variability in the response. The same procedure was repeated for numbers.

(B) The lists of letters used in the original experiment were presented again, and the subject was asked to respond by saying "UM" whatever the letter was. The variability in the time at which the signal from the throat microphone stops the counter is now reduced. Differences between the mean RTs for different letters will represent variability in the time at which a spoken signal from the tape recorder starts the counter. The same procedure was repeated for numbers.

(C) A simple "vocalization time" was found for each subject by presenting the series of "pips" and asking for the same response of "UM" to each of them. (It was found that "UM," "UP," and "UGH," all gave consistent reaction times with no differences between them. These RTs were as short or shorter than RTs for any other verbal response the experimenters tried.)

Let A be Mean RT in saying a particular letter/number to a "pip" signal.

Let B be Mean RT in saying "UM" to the same letter/number as a signal.

Let C be Mean RT in saying "UM" to "pip" signal (vocalization time).

If $C > B$ this must mean that the letter/number is being heard before the key operates to start the counter. Hence, the apparent reaction time in the main experiment is too small. The correction to add for signal variability is $(C - B)$.

If $A > C$ the key is lagging behind the subject's response and is late in stopping the counter. The apparent reaction time is too large. Hence, the correction to subtract for response variability is $(A - C)$. The corrected reaction time, RT' , for each letter/number can then be obtained from the reaction time, RT , in the original experiment:

$$\begin{aligned} RT' &= RT + (C - B) - (A - C) \\ &= RT + 2C - (A + B) \end{aligned}$$

The corrections were computed individually for each subject, for each letter/number. As an approximation, an overall correction for letters taken together and a similar correction for numbers taken together was derived. These values are shown in Table III, together with overall means for letters and numbers over the eight sessions and the final "corrected" values obtained by applying the corrections to the appropriate overall means.

TABLE III
EXPERIMENT I

OVERALL MEAN REACTION TIMES IN MILLISEC. FOR THE EIGHT SESSIONS, THE CORRECTIONS COMPUTED FROM THE CONTROL EXPERIMENT, AND THE "CORRECTED" MEANS FOR NUMBERS AND LETTERS

	Subject N. M.		Subject A. T.		Subject R. D.	
	Numbers	Letters	Numbers	Letters	Numbers	Letters
Overall mean	204	214	194	217	201	237
Correction	-13	-21	-10	-18	+13	-4
"Corrected" mean ..	191	193	184	199	214	233

For one subject (N. M.) the difference between letters and numbers is insignificant at any level, for the other two subjects the differences just border on significance at the $p = 0.05$ level. There is, thus, very little difference in reaction time produced by the difference in ensemble size.

The difference in ensemble size may be regarded as a difference between an ensemble of 26 (for letters) and an ensemble of 10 (for numbers). Since evidence will be presented later in this paper to suggest that the subject begins his response as soon as he hears the first phoneme, without waiting for the completion of the letter/number, it may be more realistic to regard the difference in ensemble size as determined by the number of different initial phonemes in pronouncing the alphabet, and in pronouncing

the numbers 0-9. The ensemble sizes would then be about 17 or 18, and 7. However, the difference in information in selecting equiprobable alternatives from these ensembles is almost the same as the difference between ensemble sizes of 26 and 10, a difference of between 1.3 and 1.4 bits.

If one regards the small differences in reaction time found in the experiment as representing real differences in the time required to cope with the different amount of information in the ensembles, despite the fact that the differences are barely significant, then a difference of about 20 millisecc. between the letter and the number condition is about the maximum one could allow. This corresponds to a rate of gain of information of between 65 and 70 bits/sec.

One way of deciding whether a real difference exists is to increase the number of ensemble sizes used. Although the difference between two adjacent ensembles may not be significant, an overall trend of increase of reaction time with ensemble size should be evident. An increased number of ensemble sizes was one of the features of Experiment 2.

EXPERIMENT 2. NONSENSE SYLLABLES

A distinction was drawn between "compatibility" and "familiarity" earlier in this paper. In the first experiment the subject was very familiar with the letters and numbers used as signals and the "compatibility" between stimulus and response was also high. A second experiment was designed to distinguish between the effects of compatibility and familiarity. The response was again to repeat back whatever was heard, but the signals were nonsense syllables drawn from groups of different sizes. Before each trial the subject was informed of the vocabulary of nonsense syllables from which signals in that trial would be drawn. Presumably at the beginning of the experiment the subject would be unfamiliar with these signals and any effect of increasing familiarity with the vocabularies would be shown as the experiment proceeded.

Method

Four different vocabulary sizes were used.

- (1) 2 choice. The signal was either DEG or WOB.
- (2) 4 choice. The signal was one of the nonsense syllables WEK, DUX, TAZ, VIP.
- (3) 8 choice. The signal was one of the nonsense syllables BEM, VUL, WAB, LIX, TOF, DUP, FEK, NAD.
- (4) Large choice. The signal could be any of the three-letter nonsense syllables, as far as the subject knew. In fact, 60 different syllables were used.

All nonsense syllables were selected from those having approximately equal association value (between 53 and 67 per cent.) in Glaze's list (Stevens, 1951). In conditions (1-3) all the nonsense syllables within any vocabulary began with a different letter (and with a different phoneme).

From each vocabulary six different lists of 20 nonsense syllables were compiled. A "controlled random" selection was used so that the frequencies of the different syllables within any one list were approximately the same. These stimulus lists were recorded on magnetic tape by A. T., with an interval of approximately 10 sec. between each syllable. Before each trial the subject was told what vocabulary would be used and, in all but the large choice condition, was given the appropriate vocabulary to learn. When he had learned it well enough to repeat it either forwards or backwards, he put on the earphones, the tape recorder was started and he was asked to repeat the syllables he heard as rapidly as possible. Reaction times were recorded in the manner already described. One practice session on an extra list drawn from each vocabulary was allowed, and this was followed by six experimental sessions (normally one per day). Four lists, one drawn from each vocabulary, constituted an experimental session and the different vocabulary conditions were presented in alternate order in consecutive sessions (1, 2, 3, 4; 4, 3, 2, 1,

TABLE IV
EXPERIMENT 2
REACTION TIMES FOR REPEATING NONSENSE SYLLABLES DRAWN FROM
GROUPS OF DIFFERENT SIZES

<i>Subject A. T.</i>								
<i>Degree of choice</i>								
<i>Trial</i>	2		4		8		<i>Large</i>	
	<i>Mean</i>	<i>S.E.</i>	<i>Mean</i>	<i>S.E.</i>	<i>Mean</i>	<i>S.E.</i>	<i>Mean</i>	<i>S.E.</i>
1	162	5.5	178	6.7	204	9.2	238	8.0
2	150	8.5	182	6.0	187	7.5	207	8.0
3	204	3.0	188	8.0	209	6.0	226	5.0
4	175	6.0	169	8.0	180	9.0	200	7.0
5	181	5.0	184	5.0	193	9.0	205	12.0
6	195	7.0	188	7.0	204	7.0	215	10.0
First three trials	172		183		200		224	
Last three trials	184		180		192		207	

<i>Subject R. D.</i>								
<i>Degree of choice</i>								
<i>Trial</i>	2		4		8		<i>Large</i>	
	<i>Mean</i>	<i>S.E.</i>	<i>Mean</i>	<i>S.E.</i>	<i>Mean</i>	<i>S.E.</i>	<i>Mean</i>	<i>S.E.</i>
1	186	10.0	213	10.0	223	10.0	247	10.0
2	182	6.8	201	5.6	225	7.7	232	13.0
3	205	3.0	207	6.4	225	6.4	250	6.1
4	172	8.0	183	9.0	187	9.0	220	7.0
5	182	5.5	192	7.5	170	6.5	223	8.0
6	166	6.0	170	7.0	181	7.5	212	11.0
First three trials	191		207		224		243	
Last three trials	173		182		179		218	

<i>Subject N. M.</i>								
<i>Degree of choice</i>								
<i>Trial</i>	2		4		8		<i>Large</i>	
	<i>Mean</i>	<i>S.E.</i>	<i>Mean</i>	<i>S.E.</i>	<i>Mean</i>	<i>S.E.</i>	<i>Mean</i>	<i>S.E.</i>
1	186	9.0	195	11.6	197	9.1	221	12.8
2	194	6.5	215	8.7	215	4.9	230	9.8
3	226	8.0	215	6.2	227	7.3	225	8.5
4	206	14.2	186	8.3	202	12.3	210	8.3
First two trials	190		205		206		226	
Last two trials	216		201		215		218	

Means and Standard Errors in millisecon. N is 20, approximately, for each entry. Overall means for the first three trials under each condition compared with the last three trials are also shown. (For subject N. M. the first two are compared with the last two since he only completed four trials.)

etc.). It should be emphasized that for each session a different stimulus list was used for each vocabulary, so there was no possibility of learning the actual lists.

The authors N. M. and R. D. acted as subjects. They knew nothing at all about the structure of the lists, nor the vocabularies used, until they entered the experiment as subjects. After a complete break of over a month, the author A. T. acted as subject using the same lists. The results are presented separately for each subject and there is no evidence to show that A. T.'s prior acquaintance with the lists gave her any advantage in predicting what was coming, although her overall performance on the first three trials is slightly better than the other two subjects.

Results

The Means and S.E.s of the reaction times for each condition are shown in Table IV. Unfortunately, N. M. only completed four out of the six sessions, but the results of these are given.

In Figure 1 are plotted (for A. T. and R. D.) the overall means of the first three trials as compared with the last three trials for the 2-, 4- and 8-choice conditions.

DISCUSSION

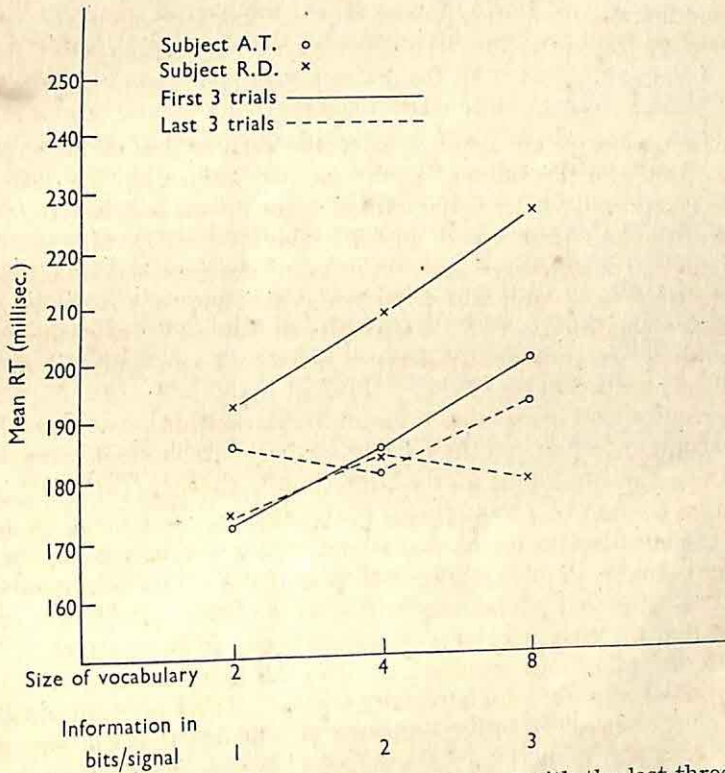
One conclusion clearly emerges. During the early stages of the experiment the mean reaction times for the different conditions vary almost linearly with the amount of information conveyed by the signal, although the rate of gain function, represented by the slope of the line, is very high (again between 60 and 70 bits/sec.). However, in the later stages, the difference between 2, 4, and 8 choice is eliminated. The only condition which shows a significant difference in reaction time from the others is the unlimited choice condition. If N. M.'s results in Table IV are examined, it can be seen that he shows no consistent difference between 2-, 4-, and 8-choice right from the start of the experiment.

It may be noticed in Figure 1 that if the linear relationship between reaction time and information content for the first three trials is extrapolated to higher degrees of choice, then the values of reaction time for the large-choice situation (Table IV) correspond to an information content of 4 to 5 bits for both subjects. This could be interpreted to mean that the effective degree of choice confronting the subject in the large-choice situation lay between 16 and 32 alternatives. If the subject began his response as soon as he heard the first phoneme of whatever nonsense syllable was presented this would mean that the effective choice was from about 20 alternatives. (Here, we are dealing with the phonemes involved in using all consonants at the beginning of nonsense syllables whose second letters are vowels. These phonemes will be different from the phonemes involved in Experiment 1.) The fact that the general magnitude of reaction times was so short suggests that the effective degree of choice was limited in some such manner. Subjects could not possibly have waited until they heard the end of a nonsense syllable before beginning their response, since the time to utter any syllable was of the order of 200 millisecc. The value obtained by extrapolation in Figure 1 supports this interpretation. In the 2-, 4-, and 8- choice conditions the fact that the choice was made from the specified number of alternatives was ensured by making the initial letter (and phoneme) of each nonsense syllable different from the initial letters (and phonemes) of all other nonsense syllables in the same vocabulary.

In a subsidiary experiment, a different group of subjects was asked to repeat monosyllabic words drawn from a set of five, without knowing which would be presented. Each word commenced with the same phoneme (e.g. Ann, Add, Amp, Axe, At). The mean reaction times obtained were all about 150 millisecc., which was the same as the reaction time to any one of them when the subject knew which would be presented.

The time to identify a word was usually longer than the reaction time. This was determined by using an "auditory tachistoscope." A series of condenser-resistance circuits was used to cut off the end of each word at a predetermined time after it began. The length of time each of the five words had to be "exposed" auditorily before it could be identified varied between 150 and 300 millisecc. This clearly establishes that most of the responses were made on the basis of the initial sounds alone. The fact that the subject begins his response on hearing the first phoneme, yet continues without hesitation to produce the correct word, must mean that the response is not fully organized from the beginning—but that some very rapid sensory-motor adjustments are taking place.

FIGURE 1
(Experiment 2)



Mean reaction times for the first three trials, compared with the last three trials, for two subjects, repeating nonsense syllables drawn from vocabularies of different sizes.

It should be emphasized that although this evidence suggests that the subjects were beginning their responses with the sound corresponding to the first letter "A," and were then modifying the ending to produce the correct word, this could not be detected by listening to what they said. Each word was pronounced rapidly and continuously, with no obvious break after the first phoneme, so that the adjustments required to produce the correct ending were very rapid indeed.

CONCLUSIONS

The results of both of these experiments seem to point towards the same conclusion. If one starts with a highly compatible stimulus-response relationship and

allows a sufficient amount of practice in the situation, there is very little significant variation in reaction time with the number of alternatives in the set from which the signal is drawn. Both Mowbray and Rhoades (1959) and Leonard (1959) reach similar conclusions, although the former authors had to use very extended practice in their light-key situation.

The present authors suggest that a distinction be drawn between two different kinds of performance, in the following manner. The conception of the human operator as a channel of limited capacity may apply only in situations where the task requires his full attention (Davis, 1957 and 1959). The human organism during the whole of its life is coping with signals from the environment which are not attended to, although responses appropriate to these signals are continually made. The transition between learning a skill and being a fully competent performer is usually accompanied by a reduction in demands on the attention of the skilled performer. The required responses are produced "automatically." Such "automatization" of responses might be regarded as a by-passing of the decision processes required to select them in the earlier stages of training. Or better, the decision making system is now programmed as a result of training, so that "automatic" selection of a response occurs. This may occur not only in tasks where there is a rigid sequence of responses, but also in situations where alternative signals require a variety of responses, provided the stimulus-response relationships are sufficiently well learned. Imitative responses of the kind considered in this experiment provide a particularly good example.

When considering applications of information theory to skilled performance it would seem worthwhile to distinguish between the "automatic" situation and the situation in which the subject has to devote his full attention to the task in order to perform it successfully. The rate of gain of information appropriate to the latter situation might be quite inappropriate to apply to the former.

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REFERENCES

- BRICKER, P. D. (1955). Information measurement and reaction times; a review. In H. Quastler (Ed.), *Information Theory in Psychology*. Glencoe, Ill.
- CROSSMAN, E. R. F. W. (1953). Entropy and choice time. *Quart. J. exp. Psychol.*, **5**, 41-5.
- CROSSMAN, E. R. F. W. (1955). The measurement of discriminability. *Quart. J. exp. Psychol.*, **7**, 176-95.
- DAVIS, R. (1957). The human operator as a single channel information system. *Quart. J. exp. Psychol.*, **9**, 119-29.
- DAVIS, R. (1959). The role of "attention" in the Psychological Refractory Period. *Quart. J. exp. Psychol.*, **11**, 211-20.
- DEININGER, R. L., and FITTS, P. M. (1955). Stimulus-response compatibility, information theory, and perceptual-motor performance. In H. Quastler (Ed.), *Information Theory in Psychology*. Glencoe, Ill.
- FITTS, P. M. (1959). Human information handling in speeded tasks. *Internat. Business Machine Corp. Res. Rep.* RC-109.
- FITTS, P. M., and SEEGER, C. M. (1953). S-R compatibility: spatial characteristics of stimulus and response codes. *J. exp. Psychol.*, **46**, 199-210.
- HICK, W. E. (1952). On the rate of gain of information. *Quart. J. exp. Psychol.*, **4**, 11-26.

- IRBY, T. F. (1956). Equivalence of estimates of channel capacity for serial and discrete tasks. *Unpublished M.A. thesis, Ohio State University.*
- LEONARD, J. A. (1959). Tactual choice reactions: I. *Quart. J. exp. Psychol.*, **11**, 76-83.
- MOWBRAY, G. H., and RHOADES, M. V. (1959). On the reduction of choice reaction times with practice. *Quart. J. exp. Psychol.*, **11**, 16-23.
- STEVENS, S. S. (Ed.) (1951). *Handbook of Experimental Psychology*. New York.

SEQUENTIAL REDUNDANCY AND SPEED IN A SERIAL TWO-CHOICE RESPONDING TASK

BY

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The task used is a self-paced 2-choice serial responding task, involving two neon lamps as signals and two response keys in simple spatial correspondence with the signals. In Experiment 1, three conditions with respectively $1/4$, $1/2$ and $3/4$ alternations between successive signals were compared. It appeared that subjects go faster with $1/4$ than with $3/4$ alternations. From the point of view of information theory, both conditions present the same amount of information per signal. The asymmetry in the effects of both forms of sequential redundancy could be attributed either to some bias in the expectations of the subjects or to some inertia phenomenon favouring responses to repeated signals. It was reasoned that if the last explanation was true, the asymmetry would be reduced if a time-lag was introduced between end of response and appearance of next signal. This was done for one group of subjects in Experiment 2. These subjects worked on the same three conditions, with a time-lag of 0.5 sec. The asymmetry was significantly reduced when compared with another group of subjects who worked with the same 0.05 sec. time-lag as in Experiment 1. Analysis of separate RTs confirmed the main conclusion that choice processes involve a transitory residual effect favouring repetitions.

INTRODUCTION

Recent experimental work on skills has made wide use of serial responding tasks where the operator has repeatedly to choose the correct responses for signals which come in rapid succession. For research purposes, such tasks present the advantage of lending themselves easily to an analysis into reaction times, since the experimenter can know exactly when the information for each response becomes available. But it must be realized that this character is presented by very few real-life situations and that in most everyday skills the relationship between receptor and effector processes is much more complicated than a simple one-to-one correspondence between isolated elements of both series.

One of the facts that complicate the picture is that in most situations, what will come can to some extent be predicted on the basis of what has come before. As Ombredane and Favergé (1955) write, man does not generally respond to isolated signals but to "signal sentences" (propositions de signaux). Now this feature can be introduced into serial responding tasks by making the series of signals sequentially redundant, i.e. by making the immediate probability of each signal dependent on which signal, or which group of signals, came before. This seems to be one way of bridging the gap between serial responding and more integrated skills.

The experimental evidence about the effects of sequential redundancy on performance is rather limited. Among the several studies relating choice reaction time to amount of information, Hyman's (1953) is the only one where the effect of varying conditional probabilities was considered. This author showed that mean RT is shorter when the series is sequentially redundant. He further contended that the

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reduction in RT is a linear function of the reduction in entropy, but his demonstration is far from conclusive since in his Experiment 3, number of alternatives was varied at the same time as conditional probabilities. Hyman was only concerned with discrete reaction times. For serial tasks all we have is the incidental observation by Crossman (1953) that in his card sorting experiments the effect of occasional bad shuffling was very marked.

A first exploration into the effects of sequential dependencies on performance in a serial task was done by the present author using card sorting. Two categories only were considered: black and red. Sequentially biased packs were compared as to sorting time with unbiased (i.e. thoroughly shuffled) ones. In all cases, overall frequencies of red and black were equal. But first-order conditional probabilities were varied. Divergence from sequential independence can be achieved in two ways: there can be more repetitions of the same colour, or more alternations between colours, than in a random series. In the first case, the occurrence of a particular colour increases the probability of its immediate reoccurrence, in the latter it decreases that probability. Equal deviations in per cent. alternations above or below 50 per cent. decrease the entropy of the series by the same amount. The main suggestion gathered from the experiments was that the effects of both deviations on sorting time are not symmetrical. Packs with $3/4$ repetitions gave shorter sorting times than packs with $3/4$ alternations.

Card sorting was, however, not a very convenient experimental situation, because (1) preparing packs of prearranged order is extremely time-consuming, (2) sudden improvements in the manipulative aspects of the skill still occur after considerable practice, and (3) there is no opportunity for recording the details of the performance. For those reasons, an apparatus providing a self-paced key-pressing task with control of first order conditional probabilities was built.

APPARATUS

The display consists of two neon bulbs, 4.5 cm. apart in the frontal plane. Two relays (G.P.O., type 3000) serve as response keys. A force of ± 80 gr. is necessary to operate them. Bulbs and keys are inserted in a horizontal panel on which the subject puts his forearms with a finger of each hand on one key. The keys are 40 cm. apart.

When either key is pressed, the stimulus light goes off. The next signal appears a constant adjustable interval after the key is released. The shortest possible interval is about 0.05 sec. The correct response consists of pressing the left key for the left signal and vice versa. Incorrect responses have the same effect on the display as correct ones, but in addition they activate an error counter.

The signals are controlled by a programming unit based on the use of two uniselectors. The principle on which it works has been described previously (Bertelson and Davidson, 1959). This programmer generates a series which is not repeated before thousands of steps. The proportion of alternations depends on the wiring of programming plug-in sockets which can be easily replaced when wanted.

The wiring of the programme sockets was chosen so that runs of identical signals of the different lengths occur with relative frequencies approaching as near as possible their probability in a Markov chain (Feller, 1950, p. 307) with the same conditional probabilities. The main departure from a true Markov chain is that very long runs never occur. In the RAND condition for instance, no run of more than seven identical signals occurs. In general, the series are more stationary than true Markov chains, since the frequency of every length is nearly constant for all successive blocks of 100 signals. This inconvenience was partially reduced by occasionally advancing the uniselectors by hand between two blocks.

The apparatus stops automatically after 50 responses. Time measures are taken on two AEG chronoscopes. One records total time per block of 50 responses. The other cumulates RTs, i.e. total time the lamps are on. (This measure was not available at the time of Experiment 1.)

EXPERIMENT I. SEQUENTIAL REDUNDANCY AND RATE OF RESPONDING

The purpose of this preliminary experiment was to answer two questions:

- (a) Can subjects take advantage of sequential redundancy to increase their response rate?
- (b) Does it make any difference whether the redundancy results from increasing the proportion of alternations above the level corresponding to randomness or from decreasing it below that level?

Three conditions were compared. In all three, left and right signals occur with equal overall frequency. They differ in the first-order conditional probabilities as follows:—

Condition REP (repeating): $p_l(l) = p_r(r) = 0.75$; $p_l(r) = p_r(l) = 0.25$.

Condition RAND (random): $p_l(l) = p_r(r) = p_l(r) = p_r(l) = 0.50$.

Condition ALT (alternating): $p_l(l) = p_r(r) = 0.25$; $p_l(r) = p_r(l) = 0.75$.

Information content is 1 bit/signal for condition RAND and 0.81 bit/signal for condition REP and condition ALT.

Method

The subjects were six Royal Navy ratings. Each subject participated in eight sessions. On the first, the experimenter demonstrated the functioning of the apparatus. He explained that the task was to deal with each block of 50 signals as fast as possible, without making errors. At each session, 20 blocks were given, the same condition being used throughout. Between two blocks, there was a rest of ± 35 sec., during which the subject was told his total time for the block and the number of errors. A rest of 3 min. was interpolated between blocks 10 and 11. The sessions were organized as follows:—

Sessions 1–2: practice on condition RAND.

Sessions 3–4–5: the three conditions, in random order.

Sessions 6–7–8: *idem*, the order for each subject being chosen independently of sessions 3–4–5.

The experimental design for the test sessions thus consisted of two successive independent random blocks.

In this experiment, the apparatus was used throughout with the minimum interval of about 0.05 sec. between release of key and onset of next signal.

On the six experimental sessions, errors were verbally penalized, according to a fixed schedule. If the subject made three, the experimenter said "Mind the errors." If he made four or more "This is too many errors. Try to avoid them." During the two practice sessions, subjects were reminded of the importance of being accurate as often as seemed necessary.

Subjects were given no verbal information about the statistical structure of the series of signals they were going to deal with.

Masking noise (± 80 db.) was applied through earphones to prevent the subject from being influenced by the rather loud clicks made by the apparatus.

Results

Mean total times per session (1,000 responses) and percentages of errors are given in Table I.

Variance analysis of the total times per subject and per session revealed a significant interaction between conditions and parts of the experiment (sessions 3–4–5 vs. 6–7–8). The data for both parts were then separately analysed, and a significant conditions effect ($p < 0.01$) was found for sessions 6–7–8 (but not for sessions 3–4–5).

Duncan's multiple range test (Federer, 1955, pp. 26-29) indicates that, at the 0.01 level of significance:

$$T_{REP} < T_{ALT} \text{ and } T_{REP} < T_{RAND}$$

but T_{ALT} is not different from T_{RAND} .

The slight differences in percentages of errors are not significant.

Discussion

It seems that, at least for subjects having an amount of practice equivalent to the first five sessions of this experiment:

- (a) a series of signals containing 3/4 repetitions permits a faster responding rate than a random series;
- (b) a series containing 3/4 alternations does not permit a better performance than a random series.

This confirms the observation made in the card sorting experiments. Response rate cannot be completely predicted on the basis of amount of information transmitted.

TABLE I
RESULTS OF EXPERIMENT I

Conditions	REP	RAND	ALT
Proportion of alternations	0.25	0.50	0.75
<i>Total time for 1,000 responses, in seconds</i>			
Sessions 3-4-5	444.5	456.0	469.8
Sessions 6-7-8	395.3	437.0	445.0
<i>Errors in per cent.</i>			
Sessions 3-4-5	2.95	3.27	4.03
Sessions 6-7-8	4.52	2.85	4.00

How can we account for the discrepancy? A reasonable assumption about the way redundancy would permit a faster performance is that subjects would, on more than 50 per cent. of the cases, prepare for the preponderant event. By preparation is meant any process leading to a gain of time on a subsequent action. Presumably, it consists of part of the operations involved in the analysis of the signal and organization of the response being done in advance. Our results seem to imply that subjects do not prepare for alternations in the ALT condition as effectively as for repetitions in the REP condition. This could possibly result from two different causes: (1) that the expectations are not symmetrical, or (2) that both types of preparations are not equally difficult.

(1) The subjects would prepare for repetitions under condition REP more often than for alternations under condition ALT, because they expect them more often. In terms which the author does not like but which are now in frequent use, the "subjective probability" of alternations under condition ALT would be lower than that of repetitions under condition REP. This hypothesis is, however, in contradiction with the well-known "gambler's fallacy" which makes people faced with a stochastic process anticipate more alternations than really occur.

(2) Due to some inertia phenomenon, the subject would, immediately after a choice, be in a better position to repeat that choice than to do the opposite one. In

condition ALT, even when he expects an alternation, it would take some time to get ready for it. The new signal comes before he has been able to prepare completely, and his RT is accordingly longer.

The latter hypothesis makes the slower response rate in condition ALT, as compared with condition REP, dependent on the time available for preparation before the onset of the next signal. A test of this hypothesis would consist of giving more time for preparation by increasing the time-lag between the end of the response and the beginning of the following signal. If the hypothesis is true, this should result in reducing the superiority of condition REP over condition ALT. Such a reduction, on the other hand, would be difficult to explain in terms of expectancy.

EXPERIMENT 2. INFLUENCE OF LENGTH OF TIME-LAG

The aim of this experiment was to test the prediction derived from hypothesis 2, that the faster response rate in condition REP when compared with condition ALT depends on the timing of signals.

Method

The same three conditions were used. There were two groups of 12 subjects (R.N. ratings again) differing in the length of time-lag between end of response (releasing of key) and onset of following signal, as follows:—

Group TL (time-lag): interval of 0.5 sec.

Group NTL (no time-lag): interval of 0.05 sec. (as in Experiment 1).

The procedure was the same as in Experiment 1: 8 sessions, 20 blocks of 50 responses per session, each of them on the same condition. The only difference is that the order of the conditions was organized in latin squares. For each part of the experiment (sessions 3-4-5 and 6-7-8 respectively) the two existing 3×3 latin squares were used, with two subjects per row. Subjects were allotted to the rows in the second part independently of the allocation in the first part. (This is in fact a mistake. It results from the fact that the experiment was started with a random block design. Then the inconvenience of this design was realized, and the last subjects were tested so as to complete the latin square design.)

The instructions were modified in order further to emphasize the need for accuracy: it was pointed out that it was even more important to avoid mistakes than to go fast.

The details of signals and responses were recorded for four blocks on each of the three last sessions, on an Evershed two-channel pen recorder. One pen was used for the signals, the other for the responses. The paper was run at a 1 in./sec. speed. From the records, RTs and pressure times could be measured with satisfactory accuracy to the nearest 1/100 sec.

Main prediction

Since overall time includes the time-lag which is different for the two groups, the comparison will be made from the point of view of RTs. Our prediction is that the difference $RT_{REP} - RT_{ALT}$ will be smaller for group TL than for group NTL. An alternative form of the same prediction is:

$$D_{REP} < D_{ALT} \quad (\text{where } D_{REP} = RT_{REP,NTL} - RT_{REP,TL})$$

The prediction is made for sessions 6-7-8, since the superiority of condition REP was demonstrated only for those sessions in Experiment 1.

Results

1. Overall scores: total time, RT and error rate

These scores for both groups on sessions 6-7-8 are given in Table II. It will be observed that total times for group NTL are practically identical with the corresponding times in Experiment 1. The error rate is lower and there is now virtually no difference between the conditions.

TABLE II
EXPERIMENT 2. RESULTS FOR SESSIONS 6-7-8

Score	Group	Conditions		
		REP	RAND	ALT
Total time per cycle in millisec.	NTL	397.2	436.8	447.1
	TL	937.3	946.0	947.1
Mean reaction time in millisec.	NTL	294.0	319.6	323.6
	TL	296.6	307.4	304.3
	D	-2.6	12.2	19.3
Errors in per cent	NTL	2.70	2.32	2.61
	TL	2.12	1.92	1.80
	D	0.58	0.40	0.81

For RTs we observe in group NTL differences which are parallel to those observed on total times. In the discussion of Experiment 1, it has been implicitly assumed that the effect of conditions on total time was reflected in the RT component. The assumption now appears to have been justified. The differences between conditions are much smaller in group TL. Variance analysis of the RTs for both groups yields a significant ($p < 0.01$) condition $\times$ time-lags interaction. The subsequent application of Duncan's test to the three values of D shows that, at the 0.01 level of significance,

$$D_{REP} < D_{ALT} \quad (1)$$

but nothing can be said about the differences

$$(D_{RAND} - D_{REP}) \text{ and } (D_{ALT} - D_{RAND})$$

Our main prediction is thus sustained: the faster responses obtained in condition REP when compared with condition ALT were, partially at least, due to the short interval between end of response and appearance of next signal.

It is worth noting that the results for sessions 3-4-5 are very similar and allow exactly the same conclusions to be reached. The differences that existed in Experiment 1 between the results of the two parts were probably due to some practice effect which the random block design did not control sufficiently.

Total times per block and corresponding cumulated RTs were plotted for each group and each condition in the form of learning curves. This permits the observation of intra-session trends. A rather surprising finding is that no improvement in RTs is observed, not even in the redundant conditions. The superiority of condition REP in group NTL is observed even in the very first block. It will be remembered that subjects were given no information about the statistical structure of the series of signals. One must thus admit that the shorter RT under condition REP cannot depend on something which must be learnt about the series of signals—unless one assumes that this learning is fast enough to be completed and give already its full

¹ Dr. M. Stone pointed out that the validity of the significance levels hangs on the supposed independence of subject interaction terms. In case this was questioned, it can be shown that for each row of the two latin squares (two Ss in each row) the differences $RT_{ALT} - RT_{REP}$ are smaller for the TL group than for the NTL group. So, by a one-tailed sign-test the significance is 2^{-6} , which is less than 0.02.

effect on performance in the first 50 responses. If one wanted to stick to an explanation in terms of expectancy, one should assume that the bias in expectancies exists prior to any contact with the series.

At the level of total times, a definite intra-session improvement is observed, in each group and under all conditions. Since practically no reduction occurs in RTs, the improvement must be due to the subjects learning to release the keys faster. This is confirmed by the consideration of inter-session trends. In Table III, total time for each session was analysed into two components: RT and a "residual time" which was obtained by subtraction and is equal to the duration of the pressure on the keys, plus the time-lag. Variance analysis shows that a significant order effect exists for residual time but not for RT.

TABLE III
EXPERIMENT 2. MEAN REACTION TIME AND RESIDUAL TIME FOR THE
EXPERIMENTAL SESSIONS (IN MILLISEC.).

Group	Score	Sessions					
		3	4	5	6	7	8
NTL	Reaction time	328	323	326	315	314	309
	Residual time	159	151	136	118	120	105
TL	Reaction time	312	310	307	307	300	301
	Residual time	669	653	649	640	647	635

2. Analysis of isolated RTs

Separate RTs were measured to the nearest 1/100 sec. from the paper records and classified as corresponding to a repeated or a new signal. Those corresponding (a) to the first reaction of each block, and (b) to false reactions, were excluded. For each condition, the median RT for each subject in each category was computed. Those medians appear in scatter-diagram form in Figure 1. Table IV gives averages for the 12 subjects in each group. The following observations are of interest.

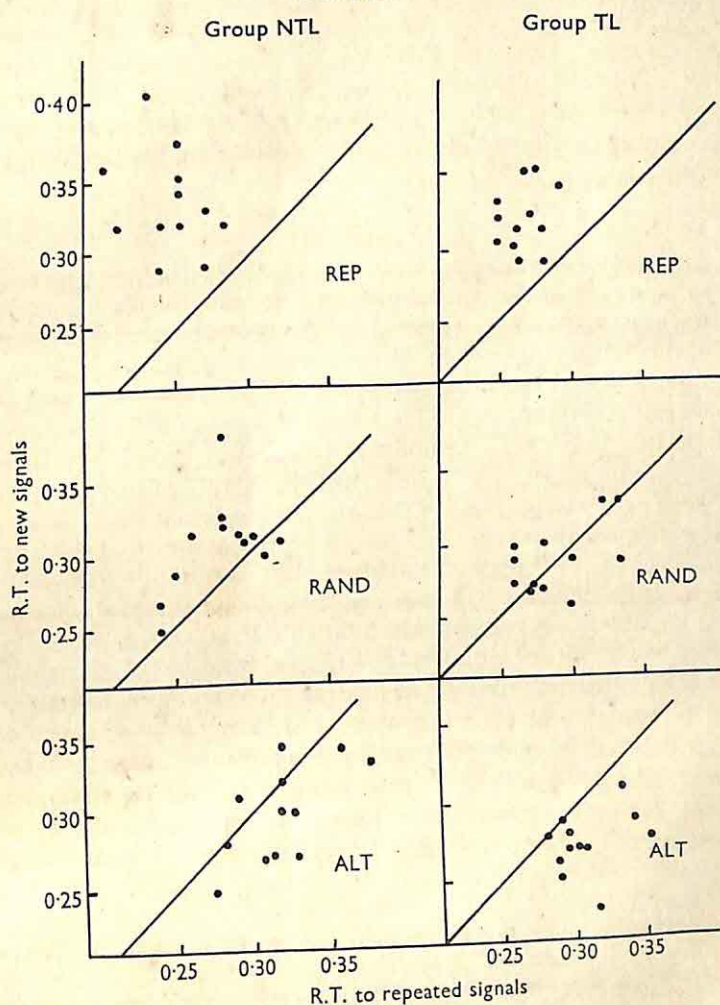
TABLE IV
EXPERIMENT 2. RT TO REPEATED AND TO NEW SIGNALS—MEANS OF
12 INDIVIDUAL MEDIANS (IN MILLISEC.)

Group	Signals	Conditions		
		REP	RAND	ALT
NTL	New	335	308	298
	Repeated	245	279	319
TL	New	320	291	273
	Repeated	272	290	307

(a) *Redundant conditions.* In the TL group, all subjects respond faster to repeated signals under condition REP ($p < 0.01$, using the Wilcoxon test, Siegel, 1956, p. 75) and all but two respond faster to new signals under condition ALT ($p < 0.01$). Subjects seem thus definitely to anticipate the preponderant event in these biased

conditions. Owing to the small differences in average RT between the two biased conditions and the unbiased condition RAND one could have wondered whether any such anticipation took place.

FIGURE 1



Scatter-diagram showing RTs to new and to repeated signals.
 Abcissa: median RT to repeated signals (in sec.).
 Ordinate: median RT to new signals (in sec.).
 Each point = 1 subject.

The subjects of the NTL group all respond faster to repeated signals under condition REP ($p < 0.01$) which is not surprising. Under condition ALT, eight respond faster to new signals ($p < 0.02$). It would thus not be true to say that there is no time to prepare for an alternation with the short time-lag of 0.05 sec. It appears that the time available for preparation is enough for the majority of subjects to reach a state of selective readiness for alternations. But it is important to notice that the state reached is much less extreme than under condition REP. This difference between the two conditions must be linked to the timing of signals, since it is not observed in group TL.

(b) *Non-redundant condition RAND.* All subjects of the NTL group but two respond faster to repeated signals ($p < 0.01$). No such tendency appears in group TL. The effect must thus be linked to the timing of signals.

For the TL group, one must notice that although there is no overall tendency to respond faster either to new or to repeated signals, individual subjects show such tendencies, either in one or the other direction. (A deviation from equality of about 10 millisec. is significant at 0.05 for those individual medians, each of which is based on 90-95 RTs). There are probably variable individual strategies favouring either repetitions or alternations. Such differences very likely play a role under the other conditions also and may account for the large scatter of individual results, which is observed in Figure 1.

DISCUSSION

One can, for ease of discussion, analyse a task like this into successive cycles, each cycle beginning with the appearance of one signal. During each cycle, the subject has two aims to achieve: respond to the present signal and prepare for the next one. In the former discussion, it was hypothesized that, in the redundant conditions, subjects would tend to prepare for the preponderant event (by "event" we mean repetition or alternation). The study of separate RTs has now shown that they actually do it. It is of course not possible to state on which proportion of the cases an adequate expectation is built up. But the fact that in both redundant conditions, and in both groups, the mean RT to the preponderant event is shorter, means that there is a tendency to prepare adequately in the majority of cases.

The main conclusion of these experiments is, however, that it takes less to prepare for a repetition of the choice which has just been made than for another choice, so that the time available for preparation becomes a critical factor. The time available for preparation must not be confused with the duration of the time-lag. Very likely, preparation can begin as soon as the reaction has been started, and proceed while the key is pressed. The possibility must of course be considered that it would occasionally be delayed by some distraction phenomenon, like the subject "paying attention" to feed-back stimuli from his own response (Welford, 1952). The amount of time available for preparation cannot be stated. It is whatever part of the total cycle is not occupied by other processes. From our present point of view, the only important fact is that it gets longer when the time-lag is made longer.

The existence of a facilitation favouring repetitions, which we propose to call "repetition effect," was assumed to explain the asymmetry in the consequences of both types of sequential redundancy we considered. It has now been shown that it can be observed also, in non-biassed series, in the form of shorter RTs to repeated than to non-repeated signals. It here manifests itself in a simpler way. With sufficient time-lag, subjects adopt varying strategies, so that some prepare for repetitions, and others for alternations. But with a short time-lag the onset of the signal catches them in a position where most of them respond faster to repetitions.

It is worth noting that a somewhat similar finding has been reported by Hyman (1953). In his non-redundant conditions, this experimenter found that RT was shorter when a particular stimulus was repeated in the series. This was observed for the 4- and 8-choice conditions, but not for the 2-choice one, where a very slight opposite tendency appeared. It will be remembered that Hyman's experiment was of the discrete reaction type. From his description, one can gather that a stimulus was presented about every 10 sec., perhaps a little less (360 measurements in a 1-hr. session). On the basis of our results, it is thus not surprising that no repetition effect was observed in the 2-choice condition. The phenomenon observed for the higher

degrees of choice can be understood under the assumption, which Hyman makes, that "the residual effect resulting from having just seen and reacted to a particular stimulus" lasts longer when there are more alternatives to choose from. A question which could also be considered is whether the effect does not last longer when a more complex coding operation is involved. The code used by Hyman (syllables to be associated to spatially arranged lights) is certainly less compatible than the very simple spatial correspondence which has been used here.

As stated so far, the repetition effect predicts two outcomes:—

- (a) When the subject expects an alternation, and this expectation is confirmed, the RT will be longer with a short time-lag than when sufficient time is available to prepare.
- (b) When he expects a repetition, and this expectation is confirmed, the RT may or may not be longer with a short time-lag—depending on whether one assumes that after a choice the subject is completely prepared for a repetition or that some preparatory adjustments must still be done—but it will be shorter than the RT to an anticipated alternation, with the same short time-lag.

This is in fact not enough to predict all the observed RTs. To achieve a detailed quantitative prediction, assumptions must still be made about the following points:—

- (1) What happens when the expectation is unfulfilled? The problem is to know whether preparation is a completely reversible process, i.e. whether the loss of time caused by an unconfirmed anticipation is equal to the time which would have been gained, had the anticipation been confirmed.
- (2) What is the relationship between insufficiency of time to prepare and subsequent increase in RT? If, e.g. 100 millisec. more had been necessary to prepare completely, is RT 100 millisec. longer?
- (3) In what proportion of cases does a subject expect each signal?

For question (2), a recent experiment by Lansing, Schwartz and Lindsley (1959) suggests that, for simple RT, curtailing the foreperiod does not produce an equal lengthening of RT: reducing the foreperiod from 250 to 50 millisec. results in an increase of about 75 millisec. only in RT (data estimated from Figure 2). Concerning question (3), it is known that many verbal guessing experiments have revealed a tendency to predict each alternative with a frequency equal to its frequency of occurrence (Hake, 1955). But it can be doubted whether these results apply to a motor skill where the expectations need not be verbalized. It has in fact been shown (Wyckoff and Sidowski, 1955) that, when guessing is only one factor in a more complex achievement, the frequency of "correct" expectations can exceed the "matching solution" level. For question (1), there are, to the best of our knowledge, no independent data on which to base a decision, and to offer an hypothesis on the basis of the present results, one should be able to answer the two other questions in a more definite manner.

Hitherto, interactions between immediately adjacent cycles have only been considered, i.e. the RT to a signal has only been considered as a function of the fact that it is identical to or different from the former signal. One could wonder whether higher order interactions should not be considered. To answer that question, the RTs were classified according to the rank of the signal in a run of repetitions, i.e. according to the number of identical signals immediately preceding it. For each subject and each condition, the median RT was obtained for each rank. The averages of the 12 individual medians for each group are plotted on Figure 2. The graphs

show, in a new form, the relationship between RT to new and to repeated signals, which has already been commented upon. But their main interest lies in the serial effects which now appear. Under the REP condition, RT still decreases beyond the first repetition. In both groups, NTL and TL, the reduction between first and third repetitions is significant at the 0.02 level using the sign test. In both groups too, it then increases again (increase between third and mean of fifth and sixth significant at 0.08). This is probably due to some expectancy phenomenon like the "negative recency effect" described by Jarvik (1951). The most puzzling fact is probably the very important decrease observed in condition ALT, for group NTL (not for group TL) on the second repetition ($p < 0.08$). It seems that the sudden occurrence of a repetition has a very marked disruptive effect under that condition. To go one step further, we classified RTs to repeated signals according to the number of alternations preceding it. We obtained a mean of 0.305 sec. after one alternation, and then, 0.320, 0.337, 0.318, 0.318 and 0.322 sec. for two and more. A repeated signal causes a disruption only when it comes after two alternations at least.

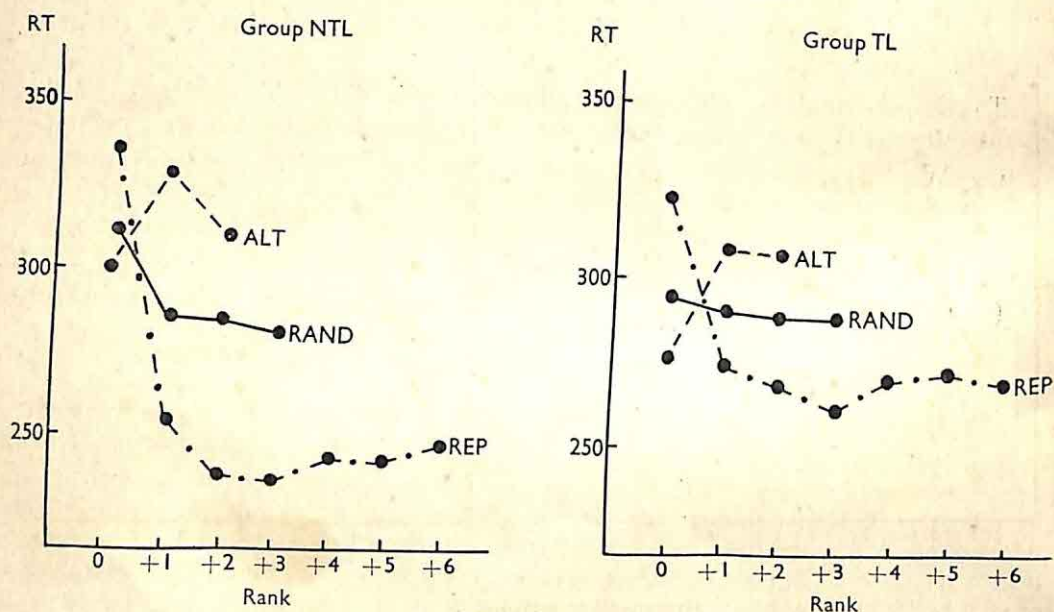


FIGURE 2

RT as a function of rank in a run of repetitions.

Abscissa: rank order (0 = alternation; +1 = 1st repetition; +2 = 2nd repetition, etc.). Ordinate: mean of 12 individual median RTs (in millisecc.).

In Figure 3 RTs have been plotted as a function of their rank in a run of alternations, i.e. to the number of alternations which occurred since the last repetition. Under condition ALT, RTs continue to decrease after the first alternation, in both groups. This is exactly parallel to what was observed for condition REP in Figure 2. The set to alternate (or to repeat) is more completely built up when a first alternation (or repetition) has already occurred. For condition REP, group NTL, the slight reduction of RT on the second alternation is non-significant.

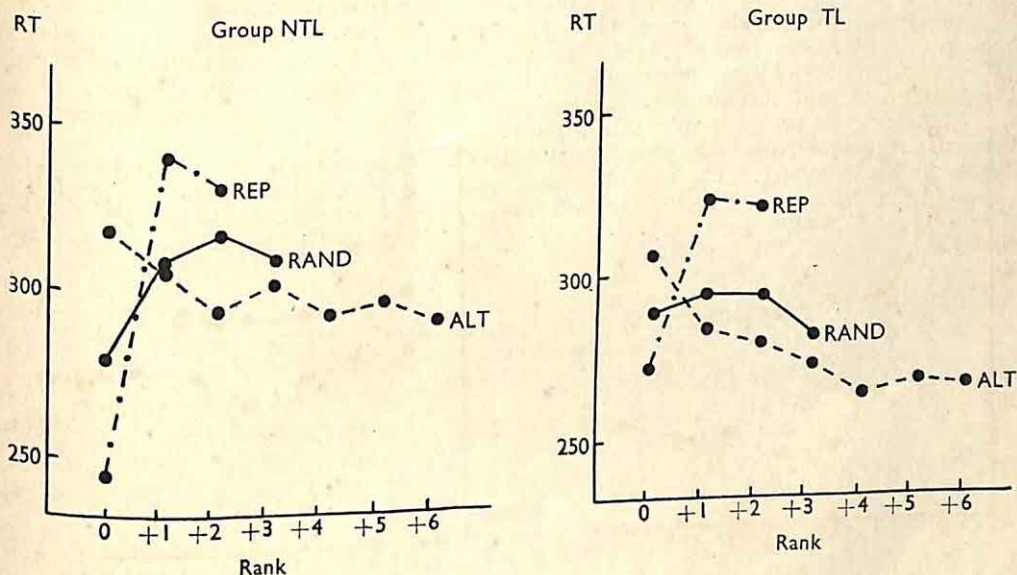
There is thus good evidence that our results cannot be explained completely by a consideration of interactions between adjacent cycles only. But these play on the whole the main role in determining these results, and in the RAND condition particularly, the influence of higher order interactions seems to be negligible.

CONCLUSIONS

(1) The effect of sequential redundancy on serial response rate depends on the timing of signals. When a short time only is available for preparation, a higher speed is attained when the redundancy results from frequent repetitions than from frequent alternations. When that time is increased by increasing the duration of the time-lag between end of response and onset of next signal, the asymmetry tends to disappear. It is not possible for the time being to state the exact limen value, but it seems to be shorter than $1/2$ sec. for the task which has been used.

(2) The phenomenon is probably due to a residual effect of choice facilitating repetitions, which we propose to call "repetition effect." This effect can be studied in a simpler way in non-biassed series, where it manifests itself under the form of shorter RTs to repeated than to non-repeated signals.

FIGURE 3



RT as a function of rank in a run of alternations.
 Abcissa: rank order (0 = repetition; +1 = 1st alternation; +2 = 2nd alternation.).
 Ordinate: mean of 12 individual median RTs (in millise.).

(3) Besides the repetition effect, which is a first-order interaction between cycles, it seems that higher order interactions also play some role—a lesser one however—in determining observed RTs.

(4) Further experiments are necessary to elucidate the origin of the repetition effect. Our present programme involves a more detailed study of the influence of length of time-lag, a consideration of the effect of S-R relationships and also of what happens with more than two degrees of choice.

The present work was started in Brussels under the direction of the late Professor André Ombredane, who, together with Professor J. M. Faverge, developed the ideas about skill which were the original stimulus to this investigation (Ombredane and Faverge, 1955).

The visit in Cambridge was made possible by a bursary from the British Council, and we are indebted to the Medical Research Council for providing excellent working facilities. Our work benefited much from valuable suggestions made by Mr. A. T. Welford, Dr. E. C. Poulton, Mr. D. E. Broadbent, and Mr. H. C. A. Dale. Dr. Mervyn Stone kindly

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REFERENCES

- BERTELSON, P., and DAVIDSON, A. (1959). A simple two-channel programming unit permitting the control of conditional probabilities. *Quart. J. exp. Psychol.*, **9**, 180-4.
- CROSSMAN, E. R. F. W. (1953). Entropy and choice time: the effect of frequency unbalance on choice-responses. *Quart. J. exp. Psychol.*, **5**, 41-51.
- FEDERER, W. T. (1955). *Experimental Design, Theory and Application*. New York.
- FELLER, W. (1950). *An Introduction to Probability Theory and its Applications*. New York.
- HAKÉ, H. W. (1955). The perception of frequency of occurrence and the development of "expectancy" in human experimental subjects. In Quastler, H. (Ed.), *Information Theory in Psychology*, Glencoe, Ill.
- HYMAN, R. (1953). Stimulus information as a determinant of reaction time. *J. exp. Psychol.*, **45**, 188-196.
- JARVIK, M. E. (1951). Probability learning and a negative recency effect in the serial anticipation of alternative symbols. *J. exp. Psychol.*, **41**, 291-7.
- LANSING, R. W., SCHWARTZ, E., and LINDSLEY, D. B. (1959). Reaction time and EEG activation under alerted and non-alerted conditions. *J. exp. Psychol.*, **58**, 1-7.
- OMBREDANE, A., and FAVERGE, J. M. (1955). *L'Analyse du Travail, Facteur d'économie humaine et de productivité*, P.U.F., Paris.
- SIEGEL, S. (1956). *Non-parametric Statistics for the Behavioral Sciences*. New York.
- WELFORD, A. T. (1952). The "psychological refractory period" and the timing of high-speed performance—a review and a theory. *Brit. J. Psychol.*, **43**, 2-19.
- WYCKOFF, L. B., and SIDOWSKI, J. B. (1955). Probability discrimination in a motor task. *J. exp. Psychol.*, **50**, 225-31.

SHORTER ARTICLES AND NOTES

ON THE RECALL OF STIMULI PRESENTED ALTERNATELY TO TWO SENSE-ORGANS

BY

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Recently Moray established that digits presented alternately to the two ears could readily be recalled in the actual order of arrival, a result which appears to show that the switching time of attention between the ears is very rapid. In the present experiments it was found that, for this type of stimulation, recall was not significantly different whether the listener knew in advance or not if the digits were to be recalled ear-by-ear or in the actual order of arrival. It was also shown that Moray's result does not apply to digits presented alternately to eye and ear.

These results are interpreted as showing that attention can select the general class of auditory stimuli rather than the particular class of stimuli on one ear: so that the apparent conclusion about switching time is not in fact correct.

INTRODUCTION

Some years ago it was shown by Broadbent (1954) that if spoken digits were presented in simultaneous pairs, one digit to each ear, and if several pairs followed each other in rapid succession, the listener had difficulty in recalling the digits pair by pair. He rather preferred to recall all those on one ear first and then all those on the other. If the interval between successive pairs was increased, the listener became able to recall the digits pair by pair. From the first of these results it follows that there is some part of the nervous system which cannot handle two digits simultaneously but which is preceded by another mechanism that can do so. Information from the latter system is selected for passage through the former (which is labelled "the P system") and the selection acts on the basis of the channel by which the information arrived. Any attempted explanation of the result must have these general characteristics.

The second result (that pair-by-pair recall is possible when presentation rate is low) admits of several possible explanations. A plausible one which has been supported by Broadbent (1954, 1958) is that the mechanism which selects information (the filter) needs time to change the channel on which it is operating. Thus the minimum presentation rate at which pair-by-pair recall occurred could be regarded as an indirect measure of the time taken to switch the filter from channel to channel. Recently it has been shown by Moray (1960) that successful recall in the actual order of presentation can, however, take place at very high presentation rates, when the two members of each pair are staggered so that signals are effectively arriving alternately on the two ears. This result implies either that switching time is much less than was previously thought, or else that for some reason switching does not occur in this particular situation. The experiments described below were intended to examine these possibilities. Moray's views on the explanation of all these effects can conveniently be discussed together with the present experimental results.

EXPERIMENT I. THE EFFECT OF PREPARATORY SET

If it were indeed true that switching time is extremely short, it would be necessary to find some reason for the superiority of ear-by-ear recall in the original experiment of Broadbent (1954): and for that matter in the repetition of that experiment by Moray (1960). A reason suggested by Moray is that simultaneously presented digits tend to interfere and that to avoid this they may be passed to two separate stores as soon as they arrive. If this is done the process of recall is then store-by-store, and thus it is easier to recall all the digits from one ear first. When the digits are presented in rapid alternation however, the tendency for them to interfere with each other is absent. In this latter case the information can all presumably enter one store on arrival: it can then be reproduced in the actual order of its arrival.

Now such a theory places great emphasis on the strategy adopted by the listener at the time when the digits arrive. It would therefore follow that a listener who received a stream of digits presented alternately to either ear, and who did not know in advance whether he was to recall them ear-by-ear or in the actual order of arrival, would find himself in difficulties. If he stored the information from the two ears separately he would do well at reproducing ear-by-ear: but if after the digits had arrived he was instructed to reproduce them in actual order of arrival, he would have difficulties similar to those met by a person who is asked to recall a long number in reverse order. Conversely, if he stored the digits in the actual order of arrival, it would be hard to reproduce them ear-by-ear. As an alternative to Moray's suggestion, we might suppose that the actual process of inserting information into store is the same, whatever the recall order is to be. The different recall orders would then affect only the process of extraction from store, and an instruction about recall order arriving after the stimuli would be just as effective as one coming earlier.

If therefore the success of Moray's subjects depends upon the adoption of a special strategy at the time when the sounds arrive, it ought to be possible to show a drop in the efficiency of recall when the listeners do not know the order of recall before they are presented with the sounds. An experiment was devised to test this possibility.

Procedure

Twenty lists each of six digits were recorded on a twin-track Ferrograph tape recorder, and presented to listeners through a split head-set (S. G. Brown, Type D1). In each list the first, third, and fifth digit were heard by the right ear, and the other digits were heard by the left ear during the corresponding silent intervals between digits on the right ear. It was found impossible to produce tapes of satisfactory quality at the rate used by Moray, and the rate actually used was one in which each channel contained only one digit, and the silence after it, per second. This is nevertheless a faster rate than that at which the subjects used by Broadbent (1954) could alternate recall between the ears.

For each list the listener received two instructions. One instruction specified whether he was to write down the list on the right or on the left of a pencil line drawn down the middle of his answer sheet. The second instruction told him whether to recall the digits in the actual order of arrival (that is, alternately from each ear) or else ear-by-ear. In the *pre-condition*, the order of recall was laid down before the list arrived, and the place to write the answer was prescribed after presentation occurred. In the *post-condition* the place of writing was laid down in advance, and the recall order after presentation. The instructions were given by the experimenter pointing to the right or the left side of the writing paper, and to a symbol representing recall order. (The instruction regarding place of writing merely served the purpose of controlling for distraction or delay due to the interpolation of an instruction before recall. This precaution is due to Brown (1960).)

Ten subjects were used, all members of the staff of A.P.R.U. Half of them received the *pre-condition* for their first ten lists, and the *post-condition* for the remaining ten. The other half received the two conditions in the opposite order. There was a break between conditions in order to explain the fresh requirements.

Results

The proportion of lists recalled correctly can be seen in Table I. It is obvious, firstly, that recall in alternating order is no harder than ear-by-ear recall. This confirms Moray's results. Secondly, the pre-condition and post-condition give almost identical results. Of the ten subjects, five gave the same score under both conditions, two do better under the post-condition, and only three do better in the pre-condition. All the latter were in the group who received the pre-condition second and were therefore more practised by the time they performed it. The difference between conditions is thus completely insignificant, the critical ratio for a test based on tau being less than one. (All subjects in fact succeeded in recalling some of the lists perfectly: two other subjects who were unable to recall any have been discarded since the equality of their performance under the two conditions means nothing.)

TABLE I
PROPORTION OF LISTS CORRECTLY RECALLED UNDER VARIOUS CONDITIONS

<i>Recall order known in advance</i>		<i>Recall order known only after stimulation</i>	
<i>Ear-by-ear recall</i>	<i>Recall in actual order of arrival</i>	<i>Ear-by-ear recall</i>	<i>Recall in actual order of arrival</i>
0.28	0.38	0.28	0.28

Because it is always difficult to interpret absence of a significant difference, we should perhaps cite a result of Broadbent (1957). He performed a similar experiment in which the two possible orders of recall were "all digits on right ear first" or "all digits on left ear first." The accuracy of recall was twice as great in the pre-condition as it was in the post-condition, at the level of practice used in the present research. This may be taken as some measure of the size of effect which occurs when recall order is indeed dependent upon the strategy adopted during presentation. Thus the present results are somewhat disappointing.

It should be added that if we take the number of individual digits recalled correctly, rather than the number of complete lists, there is a larger but still insignificant advantage for the pre-condition. In this case the critical ratio is 1.44 as opposed to 1.96 for significance at the 0.05 level. It is, however, conceivable that a larger sample of subjects might have made the difference significant: but it does not seem legitimate to consider individual digits because a listener who is prepared to sacrifice all chance of remembering the entire list (for instance, by listening only to one ear) can improve his chance of recalling at least some of the individual digits. As soon as any errors appear, therefore, we cease to know what task the listener is performing and should not rely on comparisons made on individual digits.

We have not therefore been able to find any positive evidence that the ability, to recall in the actual order of arrival signals reaching each ear alternately, depends upon a special strategy adopted at the time of presentation.

EXPERIMENT 2. ALTERNATION BETWEEN THE EYE AND EAR

As Experiment 1 does not particularly support one explanation offered by Moray for the results of Broadbent (1954), it is worth considering the possibility that switching time is indeed long and is the true explanation of Broadbent's results: but that no

switching need occur when signals arrive at the two ears alternately. In ordinary language it is possible that a man should listen, not to one ear while ignoring the other, but to both ears simultaneously while rejecting information from the other senses. Indeed this is what we usually do, since the ears normally work together and not in opposition. In formal terms, Broadbent (1958, p. 297) has defined his filter as a selective operation, "taking the form of selecting information from all sensory events having some feature in common." There is no reason to suppose that this common feature need be that of arriving by the right ear: it could equally be that of arriving by ear as opposed to eye or skin senses. Filtering on such a basis would be harmful if one stream of information were arriving at one ear and a different stream simultaneously at the other: since this would involve the entry of too much information into the P system of limited capacity. (See the first paragraph of the Introduction.) But filtering on this basis would be advantageous if the stimuli arrived alternately, since there would be no need to change the basis on which the filter was selecting and thus the time taken for such changes would be eliminated.

If such an explanation were true, it might be difficult to repeat Moray's result with a pair of sense-organs, less closely linked than the two ears are. In everyday life we often select events at "the ears" rather than "on one ear." But it is less common to use the eye and ear in strict simultaneity. Thus it might be difficult to set the filter to receive "events at the ear or eye no matter which." If so, then it would be necessary to switch the filter from eye to ear or back again between each stimulus, and recall of information in the actual order of arrival would be difficult.

On the other hand, if switching time were really irrelevant to the difficulty found by Broadbent (1954) in pair-by-pair recall, then it should surely be as easy to deal with stimuli presented alternately to ear and eye as it is with stimuli alternately to the two ears. Accordingly Experiment 2 was designed to study this possibility.

Procedure

Lists of spoken digits were recorded on B.A.S.F. editing tape whose backing will take pencil marks. (This technique also is due to Dr. J. Brown.) Three recorded digits formed each list and the interval between the beginning of one digit and that of the next was 1 sec. A paper screen was fixed to the Ferrograph playback recorder, so that the tape passing from the head to the receiving spool was visible for about $1\frac{1}{2}$ in.: that is, for half a second's running time. In addition to the auditory digits, visual ones were written on the back of the tape in such position that each became visible after the previous auditory digit had finished sounding; and the visual digit had passed completely behind the screen again before the next auditory digit occurred. Thus in each list the actual order of stimulation of the sense-organs was auditory, visual, auditory, visual, auditory, visual.

Two experimental conditions were used, differing only in the order in which subjects were asked to recall the lists of digits. In the *alternate* condition, subjects were told to write down in each list all six digits in the actual order of arrival. In the *ear-eye* condition, the three auditory digits were to be written down first and the three visual digits second. In each condition, the required order of recall was first explained and then two practice lists were given to let the subjects become familiar with the appearance of the visual digits and to ensure that the instructions about recall order were understood. Ten successive lists were then presented for response.

Twelve subjects, all Naval ratings, took part: half of them received the alternate condition first, and half the ear-eye.

Results

The proportion of lists correctly recalled was 0.59 in the ear-eye condition and 0.36 in the alternate condition. Eleven subjects did better in the former case, so that $p < 0.01$ by sign test. It is therefore clear that alternation is difficult between the ear and eye, although possible between one ear and the other.

DISCUSSION

We may now turn to a consideration of the extent to which the results of Moray (1960) and of the present paper require changes in the conclusions of Broadbent (1958). Moray makes a series of points which may be summarized without comment as follows, their validity being examined subsequently:—

- (1) In his results, slow presentation rate normally gave better performance than fast rate did: thus the improvement in pair-by-pair recall as rate was slowed (Broadbent, 1954) need not be due to switching time, but rather to a simple effect of rate upon recall.
- (2) The time taken to perceive one digit is uncertain because digits are recognizable even if the initial part only of the word (less than 100 millisecc.) is heard. It may well be less than 250 millisecc., because it is possible to carry out an ordinary digit span with a presentation rate of 4 digits/sec. For this reason it is thought incorrect to regard (as Broadbent does) the possibility of pair-by-pair recall, when digits are presented at $1\frac{1}{2}$ sec./digit/ear, as indicating that $1\frac{1}{2}$ sec. is the minimum time for two perceptions and two switching times. This would mean switching times of over half-a-second each, which are absurd.
- (3) It is in any case difficult to measure switching time because of the possibility that the listener may avoid switching by selecting both channels at once. It is also possible, as an alternative to Broadbent's model, to suppose that both sets of digits are passed simultaneously through the filter to separate stores and that they are then reproduced store-by-store.

The first three points he regards as showing that mutual interference, rather than rate, explains the inefficiency of listeners in dealing with simultaneous digits: and also as casting doubt on the existence of a short-term store peripheral to the filter.

- (4) He then reports an experiment on ear-by-ear recall of stimuli presented as by Broadbent (1954), but at varying rates, and points out that corresponding results only for pair-by-pair recall were given in that paper. The results show good performance at slow rates, which is regarded as contrary to Broadbent's explanation since the short-term store should have more time to decay at slower rates. Analysis of errors on individual digits also shows that the first digit on the second ear is not the worst recalled, as it should be if it has been in store longest. These data also are interpreted as casting doubt on the existence of a store before the filter.
- (5) Lastly, the frequent occurrence of transposition of, say, the first digit on one ear and the first on the other, is regarded as a difficulty for Broadbent's view.

These points are of varying merit, and may be criticized as follows: the comments being numbered to correspond with the points to which they refer.

- (1) It is only when digits are arriving by two separate sensory channels that slow presentation is superior to fast. In simple memory spans the exact opposite is true (Conrad, 1957; Fraser, 1958). (Pollack (1952) reported the same finding for the speeds and vocabulary sizes here in question, although the result was reversed at still slower speeds and for smaller vocabularies). Thus the better performance at slow presentation rates found by Moray cannot be attributed to a simple effect of rate on memory, but must be connected with the use of two channels. The obvious explanation is that

time is used in switching between channels, and no other explanation has yet been produced.*

(2) It is not true to argue that, because a sound lasts only a certain time, perception only takes that time. In the case of vision, it has long been recognized that the minimum duration of the tachistoscopic exposure, necessary for identification, is not a measure of perception time because the incoming information may be processed for some time before identification is achieved. (See for example Vernon, 1952.) On the other hand, a succession of sensory events may accumulate over a certain minimum period before they are passed through the identifying mechanism (Broadbent, 1958, p. 278 f. More recent evidence on this view, now facetiously called the lavatory cistern theory, is given by Broadbent, 1960). In such a case a rapidly presented digit span would secure the arrival of two or more digits within each sensory sample: but the length of the latter, rather than the length of time occupied by one spoken digit, would decide the minimum time spent on any one sensory channel. The data which Moray cites are thus irrelevant to the question. At the same time, his point that switching time cannot be determined until perception time is known, and that the latter is completely uncertain, is a good one.

(3) Some careful distinctions must be made here. In so far as Moray is suggesting that a different method of storage is used for signals arriving simultaneously as opposed to alternately, Experiment 1 of the present paper has failed to provide definite supporting evidence for him. On the other hand, in so far as he suggests that the information on the two channels is first stored simultaneously in two places, and that the two stores are then emptied one at a time, he is in complete agreement with Broadbent's model and has not put forward an alternative view. Indeed, as was said at the beginning of this paper, the results of experiments on simultaneous presentation entail a theory of this type, and it is not surprising therefore, that Moray's model is similar. Instead of using the word "filter" to correspond to the mechanism which selects information from the simultaneous stores, however, Moray terms this process "reproduction" and transfers the word "filter" to some other process (undefined) which precedes the storage. His conclusion that his evidence casts doubt on the existence of a short-term store before "the filter" is therefore a purely verbal one, having no reference to the word "filter" as used by Broadbent.

There is probably, however, an implied suggestion in Moray's usage that the process of filtering (in Broadbent's sense) occurs at the time of recall and not at that of presentation. This possibility was raised by Broadbent (1957) and some experiments carried out to test it. The conclusion was that listeners can indeed delay the transfer of information from the simultaneous to the successive arrangement, but that this is an inefficient strategy, and that listeners therefore learn to abandon it. They do better if they filter the digits as soon as they are presented. For this reason it is most unlikely that Moray's subjects were actually filtering at a time much after the presentation of information.

(4) Other experiments given by Broadbent (1957) concerned the effect, on ear-by-ear recall, of presenting digits at varying speeds, using unequal numbers of digits on the two ears and varying the position of the digits on the ear which was second in reproduction. The conclusion from that experiment was that, at the speed used by Moray, the listener did not filter the incoming information in the way which the

* Since this paragraph was written, one of us (D. B.) has visited the U.S.A., and been told of several unpublished experiments showing that a slow presentation rate does not impair recall if rate of recall is itself fixed. The impairment found for slow presentation with free recall may in that case be due to a slower recall. This considerably weakens our argument.

instructions required. He had a tendency to listen first of all to digits on the wrong ear, then to switch to the correct ear, and then in reproduction to invert the order of recall. This view was supported by a significant change in the order of difficulty of different digits in the list. As a result Broadbent concluded that it was not possible to gain any valid information from a slowing-down of the rate of presentation of digits presented simultaneously for ear-by-ear recall. This conclusion naturally applies to Moray's experiment: it is highly probable that his subjects were not listening to the digits in the order required by the instructions, and this view is supported by the order of difficulty of different digits in the list. Thus the good performance which he found at slow presentation rates does not imply that the temporary store does not decay rapidly.

(5) The frequent occurrence of transpositions between the ears also provides evidence that the instructions had failed to control the filtering of information by the listeners. Such transpositions naturally tend to occur if the listener selects the first digit from one ear and the last two from the other, and then retrieves the remaining digits from store.

It can be seen from a survey of these points that Moray's evidence is in some cases inconclusive and that in the remainder his suggestions agree with those put forward earlier by Broadbent, with some differences of nomenclature. It is common ground, that, when digits are presented simultaneously to the two ears, they are stored simultaneously in different places and then those at one ear are selected before any of those on the other ear. When digits are presented alternately, it would seem that the process of selection can be either of the same type or of a type in which both ears are selected at the same time. Thus the digits can be recalled equally well either ear-by-ear or in the actual order of arrival. The latter strategy is not usable when digits are presented simultaneously, because of what Moray calls "interference" and Broadbent refers to as "the limited capacity of the P-system." Nor can it be used when the two sensory channels are the eye and the ear rather than the right and left ears (Experiment 2). Presumably this is because the selection mechanism ("filter" to Broadbent but not apparently to Moray) cannot select both senses at once whereas it can select both ears at once.

Moray's results do not therefore require any abandonment of views previously held: but they add something to our knowledge of the physical characteristics which can be used by attention as a basis for its selection of some sensations as opposed to others.

Many of the ideas here presented arise from conversations with N. Moray and J. Brown, although they may have deteriorated in transmission.

REFERENCES

- BROADBENT, D. E. (1954). The role of auditory localization in attention and memory span. *J. exp. Psychol.*, **47**, 191-96.
- BROADBENT, D. E. (1957). Immediate memory and simultaneous stimuli. *Quart. J. exp. Psychol.*, **9**, 1-11.
- BROADBENT, D. E. (1958). *Perception and Communication*. London.
- BROADBENT, D. E. (1960). Time to react. *Advanc. Sci.*, **17**, 21-5.
- BROWN, J. (1960). Evidence for a selective process during perception of tachistoscopically presented stimuli. *J. exp. Psychol.*, **59**, 176-81.
- CONRAD, R. (1957). Decay theory and immediate memory. *Nature*, **179**, 831-2.
- FRASER, D. C. (1958). Decay of immediate memory with age. *Nature*, **182**, 1163.
- MORAY, N. (1960). Broadbent's Filter Theory: Postulate H and the problem of switching time. *Quart. J. exp. Psychol.*, **12**, 214-20.
- POLLACK, I. (1952). The assimilation of sequentially encoded information. 2. Effect of rate of information presentation. HRRL Memo Report No. 25, U.S.A.F.
- VERNON, M. D. (1952). *A Further Study of Visual Perception*. London.

A NOTE ON BROADBENT AND GREGORY'S PAPER "ON THE RECALL OF STIMULI PRESENTED SIMULTANEOUSLY TO TWO SENSE ORGANS"

BY

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My thanks are due to the Editor for allowing me a chance to comment on Mr. Broadbent's paper. I would like to make four points.

1. *The effect of rate of presentation*

On the matter of the estimation of the "perception time," I would still maintain that if in a "conventional" (single-channel) immediate memory span experiment a person is capable of taking in signals at the rate of 4 per sec., then the time for the perception of a digit must be very much less than the $\frac{1}{2}$ sec. which Broadbent originally gave as his estimate. Without further developing this idea, I do not think that his arguments about a sampling time during which more than one signal may be accepted throws any light on the problem, although it certainly opens up interesting lines of thought. If we are faced now with a situation where we could make the subject receive 2, 3 or 4 signals as a single input if they were of short enough duration, it would seem that we should completely re-interpret the model using this sampling time as the unit of temporal measurement rather than using seconds: for the estimates of the effects of rate of decay, etc., must now be made with respect to the durations of sampling times, not with relation to the times of onset and ending of the individual signals.

2. *Rate of presentation and rate of decay of signals*

My argument here may disappear if the concept of time-sampling of incoming signals is developed and some estimation of the actual duration of the samples is offered and turns out to be of the correct order. But at present I feel that Broadbent has not, in his paper, dealt with my point that as things stand the number of errors which subjects make are too small, if we stick to the written and verbal estimate of the decay time of items in the short term store which he has given and which I have quoted elsewhere. If the subject listens to two simultaneous trains of signals by first accepting those to one ear and then those to the other ear, and the time constant of the store for the "held up" signals is about a second, then the following must hold. At a rate of presentation of 2 signals/ear/sec. it will take more than 1 sec. to present three signals to each ear, assuming that perception of a digit does not take longer than the digit itself lasts. If, because of time sampling the perception time is greater than the duration of the signal, then the time will be longer still. This means that the first signal to be "held up" on the ear which is listened to second must stay in store for at least 1 sec., and at worst rather longer. If the decay constant of the store is about 1 sec. we would therefore expect an error rate approaching 1 for each group of signals when they are presented at a rate of 2/ear/sec. In all my experiments I have found mean error rates of the order of 0.2 to 0.3 errors per group of digits as far as the digit occupying the position of the first of the "held up" signals is concerned. This is far too low for Broadbent's model.

3. *Comparison of inter- and intra-sensory switching*

Broadbent's work clearly shows that there is a difference in switching between the two ears and between the ears and the eyes. This I would expect, as I feel that it is very unlikely that a model based largely on one modality will be applicable to another. The fact that it is so much easier to listen to one ear than to look with one eye is evidence for a difference of organization of the two senses. This is underlined by neurological work such as that of Hernandez-Peon, Scherrer and Juvet (1956) on cats whose attention switched from hearing to looking. The peripheral suppression of auditory input found in that study is most unlikely to be operating in selective listening to dichotic stimuli, as I have argued elsewhere (Moray, 1960a). I do not feel that experiments on inter-modality phenomena can be used as criticism of work based on intra-modality phenomena.

4. *Interpretation of Broadbent's model*

On the basis of various discussions, I would now align myself more nearly with Broadbent's position. The experiments he reports above I would accept as invalidating my suggestions to which they refer. I think that I could accommodate them without too much "post hoc"-ing; but it seems more important to point to the features of Broadbent's description of his model which first were responsible for my experiments and criticisms. Firstly (Moray, 1960b) I still feel that there is a logical looseness in trying to argue from the mode of presentation of stimuli as we do it at present to the strategy which the subject must be adopting. More important, I feel that there is a considerable ambiguity in Broadbent's original (1958) presentation of his model. The impression given is that the short-term store (S-system) prior to the filter is distinct from the store which handles material in a conventional, single-channel, immediate memory span experiment. The latter seems in the model to consist of a circuit including the P-system: feedback: S-system: filter: P-system, and the S-system is really only needed to deal with a situation where either because of multichannel input or because of input plus feedback the P-system becomes overloaded. If the S-system is in fact part of the memory system used in conventional immediate memory span experiments then my objections as to its redundancy would in part be met; although I would still be suspicious of the storage for a time of the order of a second or more of material previous to a perceptual system. Recent work by Averbach and Sperling (1960) for example cannot be used to support the idea since his effects seem more closely related to after-images than to storage in the usual sense of the term. Perhaps the time sampling suggested by Broadbent would cover the point, in which case is it assumed that the S-system functions as a sort of collecting box for the time-sample?

On the difference in the use of the word "filter" between Broadbent and myself, the following distinction seems clear. I used it more in the sense of a mechanism which re-directed incoming signals along different paths within the system, and did not merely block certain paths, whereas Broadbent restricts his use of it more exactly to the latter.

REFERENCES

- AVERBACH, E., and SPERLING, G. (1960). Short term storage of information in vision. *4th London Symposium on Information Theory*. London.
- BROADBENT, D. E. (1958). *Perception and Communication*. London.
- HERNANDEZ-PEON, R., SCHERRER, H., and JOUVET, M. (1956). Modification of electrical activity in cochlear nucleus during "attention" in unanaesthetized cats. *Science*, **123**, 331.
- MORAY, N. (1960a). *Studies of Selective Listening*. Unpublished D.Phil. thesis for University of Oxford.
- MORAY, N. (1960b). Broadbent's Filter Theory: Postulate H and the problem of switching time. *Quart. J. exp. Psychol.*, **12** 214-20.

PARTIAL REINFORCEMENT IN THE TURTLE

BY

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Two matched groups of young turtles, *Pseudemys scripta elegans*, were trained in a simple runway, one with partial and the other with consistent reinforcement, following which both were extinguished. The two groups did not differ significantly in resistance to extinction. This finding constitutes further evidence against the hypothesis that the paradoxical effect of partial reinforcement which is found in higher animals is a perfectly general one, to be understood in terms of the simplest principles of learning.

INTRODUCTION

Recent work with the fish has failed to show the paradoxical effect of partial reinforcement on resistance to extinction which is characteristic of the rat and certain other mammals. In a series of experiments with a food-rewarded instrumental response, the initial resistance to extinction of partially reinforced African mouth-breeders was found to be significantly *less* than that of consistently reinforced controls under a variety of conditions—after little or after much training, with massed or with spaced practice, and with high or with low drive (Wodinsky and Bitterman, 1959; Longo and Bitterman, 1960; Wodinsky and Bitterman, 1960). In an experiment on classical conditioning, the resistance to extinction of goldfish was found to be *unaffected* by partial reinforcement, whether after little or after much training (Gonzalez, Longo and Bitterman, 1961). These findings seem to require a new attitude toward the mammalian data, with which learning theorists have attempted to deal in terms of the very simplest principles, such as stimulus-generalization (Sheffield, 1949) or habituation (Weinstock, 1954), implying that the paradoxical effect is a perfectly general phenomenon, discoverable in any animal capable of learning. Our work with fish casts doubt on the phyletic generality of the paradoxical effect and suggests that it is due to some more specialized process, valuable clues to the nature of which might reward a systematic search for the effect at other points in the animal series.

In the experiment to be reported here, the relation between consistency of reinforcement and resistance to extinction was studied in the turtle, *Pseudemys scripta elegans*. Since our facilities for maintaining larger animals had not yet been developed, the work could be done only with immature specimens, which introduced an ontogenetic as well as a phyletic dimension of variation, but ability to learn is all that is required for a further test of the assumption that the paradoxical effect is to be understood in terms of the most elementary processes. Furthermore, differences due to maturity, as well as differences between species, may be expected ultimately to contribute to the analysis of the problem.

Knowing very little about the control of drive in our turtle, we thought it safest to work with a 24-hr. intertrial interval. Such an interval permits the use of a substantial amount of reward on each trial without danger of satiation. In the rat, partial reinforcement increases resistance to extinction when the intertrial interval is 1 day (Weinstock, 1954), just as it does when practice is massed. In the African mouthbreeder, partial reinforcement decreases resistance to extinction when the

intertrial interval is 1 day (Longo and Bitterman, 1960), as it does when practice is massed.

METHOD

The subjects were 24 young "sliders," between 5 and 6 months old, obtained from a local dealer. Pairs of animals were kept in standard 2-gal. aquariums, which were arranged to provide facilities both for basking and for swimming. The subjects used were selected from a somewhat larger group (of about 30 animals) on the basis of their superior adjustment to laboratory conditions as indicated by general health and appetite.

The experimental apparatus was a straight alley, 2 in. wide and 18.5 in. long (inside dimensions), made of clear $\frac{1}{4}$ -in. plastic. The floor of the alley was level for a distance of 15 in. from the starting end, after which it sloped downward at an angle of about 20°; the depression was filled with water to provide a feeding place. A guillotine door, which the experimenter controlled by manipulating a light thread, was situated 12 in. from the starting end of the runway and could be lowered to prevent retracing from the feeding area once it had been entered. Pivoted above the water-filled depression at the far end of the runway was a thimble which contained the reward (dried "ant eggs"). By manipulating another thread, the experimenter could drop the reward into the water after the guillotine door had been lowered behind the animal. The purpose of this arrangement was, of course, to rule out the possibility that the response of the animal on any trial would be influenced by cues arising from the presence or absence of reward. The two long walls of the apparatus and the short wall at the starting end were painted light gray (exterior surfaces only), while the short wall at the goal-end remained clear to enhance the opportunity of the animal for orientation within the runway. The floor was black.

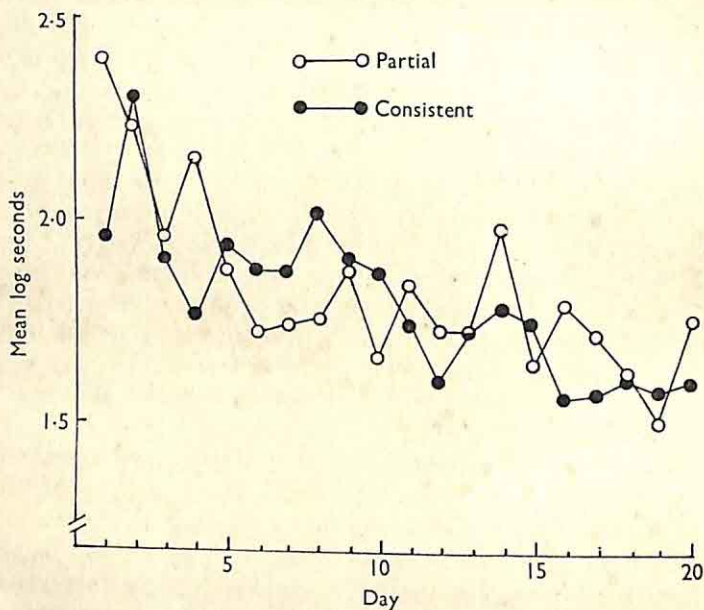
After several weeks of adjustment to life in the laboratory, during which time a 24-hr. feeding cycle with a diet of "ant eggs" and ground beef was instituted and the animals were adapted to handling, training in the runway was begun. Each animal was given one trial per day. Taken from its living tank, it was put down at the start of the runway, and a stopwatch was started. When the animal reached the feeding depression, the watch was stopped, the guillotine door was lowered, and the reward (5 "ant eggs") was dropped into the water. After eating the reward, which required about 30 sec., the animal was returned to its living tank.

In the first stage of the experiment, which lasted 20 days, every response was reinforced. On the basis of their performance during this period, the animals were paired to form two matched groups of 12 animals each. The two groups were treated differently in the second stage, which lasted 56 days. One group—the Consistent Group—was rewarded on every trial as before. A second group—the Partial Group—was rewarded only on half the trials, reinforced and unreinforced trials following each other in irregular sequence (Gellermann, 1933). The procedure on unreinforced trials was exactly the same as on reinforced trials except that the thimble was empty, delivering no food when tilted, and the animal simply was detained in the goal-end for a period of 30 sec. On unreinforced days, the animals in the Partial Group were given 5 additional "eggs" during the daily feedings which followed the experimental sessions by about an hour; the total daily intake of the two groups thus remained the same. In the third stage of the experiment, both groups were extinguished, the procedure on all trials being exactly the same as on the unreinforced trials for the Partial Group during the second stage. If on any trial an animal failed to reach the goal-box in 5 min., it was removed from the apparatus and the trial was terminated, a running time of 300 sec. being recorded. The third stage of the experiment began on Day 77 and lasted until Day 113, whereupon the work was concluded.

RESULTS

In Figure 1, the results for the first stage of the experiment are plotted in terms of mean log time per trial. The performance of the two groups was very much the same, which is not surprising because the groups were treated alike and were in fact matched on the basis of that performance. The learning curves, which resemble those obtained routinely with rats in analogous situations, seem quite close to asymptote by Day 20.

FIGURE 1



The course of learning in the first stage of the experiment.

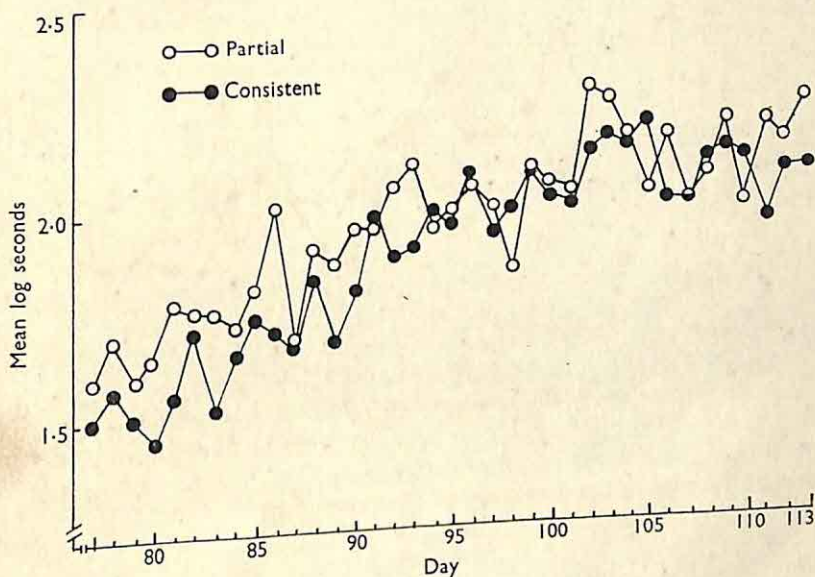
In the second stage of the experiment, the two curves separate, that for the Consistent Group remaining at the level achieved in the first stage, and that for the Partial Group tending to rise to a slightly higher level. The mean time per trial for all days of the second stage combined was 1.54 log sec. for the Consistent Group and 1.69 log sec. for the Partial Group. Tested by a non-parametric method for paired replicates (Wilcoxon, 1949), the difference falls considerably short of statistical significance.

Extinction curves plotted in terms of mean log time per trial are shown in Figure 2. Again in extinction the performance of the two groups was very much the same. The mean time per trial for all extinction days combined was 1.93 log sec. for the Consistent Group and 1.89 log sec. for the Partial Group. Tested by Wilcoxon's method, the difference falls far short of statistical significance. A second measure of resistance to extinction was provided by the number of response-failures—that is, the number of extinction trials that were terminated because the animal failed to reach the goal-box in 5 min. The mean number of failures was 4.6 for the Consistent Group and 4.9 for the Partial Group, the difference being quite insignificant statistically.

DISCUSSION

It is dangerous, of course, to argue from a null outcome. Where, as in our earlier work with the African mouthbreeder, resistance to extinction is found to be greater after consistent than after partial reinforcement, one can speak with some confidence of a functional difference between the new animal and the rat, but a null outcome may stem simply from unreliability of measurement. The possibility remains that an improved technique might reveal an effect like that found in the rat. It should be noted, however, that the technique used here for the turtle was adequate to produce highly reliable acquisition and extinction functions. Furthermore, the paradoxical effect found in the rat is such a striking one that an experiment which was crude indeed would be required to mask it, and there is little reason to assume that the present experiment was so crude.

FIGURE 2



The course of extinction in the third stage of the experiment.

Nevertheless, the issue will not be resolved by a single experiment. Further work on the turtle under a variety of conditions and with different patterns and probabilities of reinforcement is required—work such as that which has been done with rat and fish. From the point of view of interspecific comparison, these experiments might best be done with mature specimens, since the possibility exists that the paradoxical effect depends on processes which appear only later in the life of the animal. From the developmental point of view, it might be interesting to make systematic comparisons of younger and older specimens.

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REFERENCES

- GELLERMANN, L. W. (1933). Chance orders of alternating stimuli in visual discrimination experiments. *J. genet. Psychol.*, **42**, 206-8.

- GONZALEZ, R. C., LONGO, N., and BITTERMAN, M. E. (1961). Classical conditioning in the fish: Exploratory studies of partial reinforcement. *J. comp. physiol. Psychol.*, **54**, in press.
- LONGO, N., and BITTERMAN, M. E. (1960). The effect of partial reinforcement with spaced practice on resistance to extinction in the fish. *J. comp. physiol. Psychol.*, **53**, 169-72.
- SHEFFIELD, V. F. (1949). Extinction as a function of partial reinforcement and distribution of practice. *J. exp. Psychol.*, **39**, 511-26.
- WEINSTOCK, S. (1954). Resistance to extinction of a running response following partial reinforcement under widely spaced trials. *J. comp. physiol. Psychol.*, **47**, 318-22.
- WILCOXON, F. (1949). *Some Rapid Approximate Statistical Procedures*. New York, 1-16.
- WODINSKY, J., and BITTERMAN, M. E. (1959). Partial reinforcement in the fish. *Amer. J. Psychol.*, **72**, 184-99.
- WODINSKY, J., and BITTERMAN, M. E. (1960). Resistance to extinction in the fish after extensive training with partial reinforcement. *Amer. J. Psychol.*, **73**, 429-34.

VISUAL DISCRIMINATION OF HORIZONTAL AND VERTICAL RECTANGLES BY RATS ON A NEW DISCRIMINATION TRAINING APPARATUS

BY

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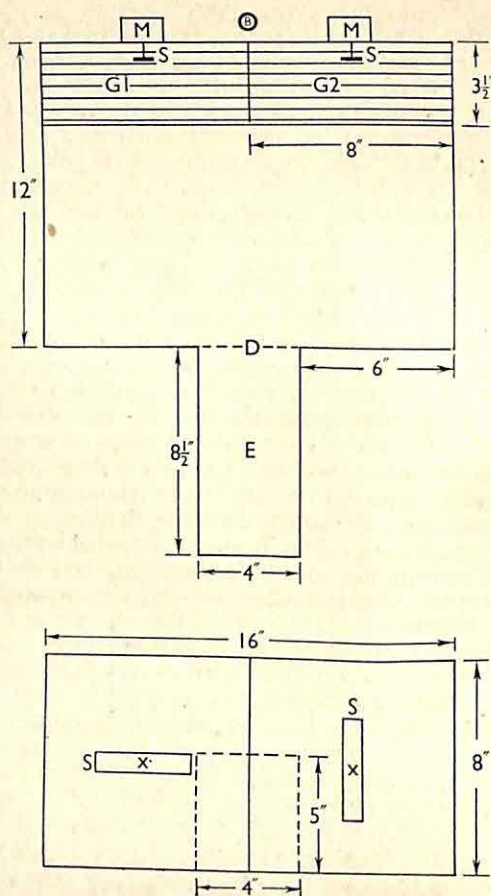
A new visual shape discrimination apparatus for use with rats is described. Rats were trained on a simultaneous discrimination using stereometric shapes, and the reward was milk which was delivered through the middle of the shapes. Animals were trained to discriminate vertical and horizontal rectangles on the apparatus, and it was found that they took as many trials to learn as on a Lashley jumping stand. Because of the speed at which individual trials may be conducted, less actual time is spent on training in this apparatus than in a Lashley jumping stand. Some transfer tests were conducted and indicated that: (1) although the bottom halves of the shapes were more important for rats than the top halves, animals had learned something about the shapes as a whole; (2) animals transferred to discontinuous shapes; (3) they transferred better to larger shapes than to smaller; (4) they had learned both to approach the positive shape, and to avoid the negative shape.

Recent evidence on visual discrimination training in monkeys indicates that to obtain rapid learning the following three conditions should be satisfied: (1) reward and the object to be discriminated should be spatially contiguous; (2) the discriminatory response should be made to the object to be discriminated (McLearn and Harlow, 1954; Murphy and Miller, 1955, 1958); (3) the discriminanda should be stereometric (Harlow and Warren, 1952; Weinstein, 1941). These three principles were applied in an attempt to develop an improved apparatus for visual discrimination training in rats.

APPARATUS

Scale plans of the discrimination box are shown in Figure 1. Walls were made of metal and painted black. The floor of the apparatus was of wire mesh, except for the two grids (G1 and G2) which were brass bars insulated from the remainder of the box. The shapes used as discriminanda (S) were cut out of $\frac{1}{4}$ in. white perspex; they were attached to the rear wall which was of steel by means of magnets fixed to the back of each shape: this facilitates rapid changing of the shapes. Two $\frac{1}{4}$ in. holes were drilled in the rear wall at the points marked X: a drinking tube protruded through each hole, and emerged through corresponding holes cut in the centre of each shape. The nozzles projected about $\frac{1}{4}$ in. beyond the shape, and were set at an angle of about 10° to horizontal to prevent the formation of air bubbles in the tube. The tubes were supplied with milk from bottles suspended behind the rear wall (M). The grids were wired up so that when a rat stepped on them a bell (B) sounded: by throwing a switch the experimenter could set the bell to sound when the rat trod on the left-hand grid or to sound when it trod on the right-hand grid. A vertically sliding glass door (D) was in position at the exit from the starting box (E). The starting box had a hinged transparent perspex lid. Since rats did not attempt to escape from the discrimination chamber there was no need for a lid to that. The apparatus was illuminated by a 60 watt bulb suspended 24 in. above the starting box.

FIGURE 1



Scale drawings of ground plan and frontal elevation of apparatus. The dashed line in the frontal elevation represents the start box.

TRAINING

Eight naive 6 month old female hooded rats were trained to discriminate between vertical and horizontal 10×2 cm. rectangles. Four rats were trained to approach a vertical rectangle, four to approach a horizontal. Rats were kept on a $22\frac{1}{2}$ hr. feeding schedule throughout the experiment. They were trained initially on a simpler form of the apparatus described above, and the latter was in fact designed to overcome possible flaws in the initial training apparatus. The initial discrimination box had a similar rear wall to that shown in Figure 1, though overall dimensions were smaller. It had no grids, no bell and no start chamber and the walls were of wire mesh not sheet metal. Rats were first left in the box for an hour in groups of two with the positive shape only in place. The shape was moved from one side to the other every 10 min. All the animals learned to drink milk from the tube projecting through the shape. On the following day training commenced. Rats were given 10 trials a day with 5 min. between each trial. A modified correction procedure was used. Any approach to within approximately 2 in. of the manually from the box was counted as a mistake. When a rat made a mistake, it was removed it made a correct choice. When a rat made a correct choice, it was allowed 9 sec. drinking shapes. The position of the shapes was changed from trial to trial following Gellerman type sequences (Gellerman, 1933).

It would appear that learning in this apparatus does have two advantages: (1) very little pretraining is needed; (2) individual trials take less time: since rats run very quickly on this apparatus, each trial takes about 30 sec. only and this is true from the very start of training.

TRANSFER TO OTHER SHAPES

It was thought of some interest to check previous findings on transfer with horizontal and vertical shapes in this apparatus. The transfer test shapes used are shown in Figure 2. During transfer test trials the bell was switched off and rats were allowed to drink for 9 sec. from whichever shape they selected. Transfer test trials were interspersed with retraining trials on the original shapes. The first pair of shapes shown in Figure 2 are the original training shapes: animals averaged 95 per cent. correct responses on the retraining trials with these shapes, and this gives a base line from which to measure transfer to other shapes. The remaining six pairs of shapes are the transfer shapes used: the left-hand member of each pair was treated as a vertical and the right-hand member as a horizontal. Thus a "correct response" as a vertical and the right-hand member of a pair in Figure 2 for a rat trained in transfer was a run to the left-hand member of a pair for a rat trained with the vertical rectangle positive and to the right-hand member for a rat trained with the horizontal rectangle positive, and percentage of "correct responses" for each pair of transfer shapes was computed accordingly. Findings are in line with previous results. There was more transfer to large shapes than to smaller shapes (tests 1, 2); there was good but not complete transfer to discontinuous shapes (tests 5, 6), even where the elements in the discontinuous shapes were small rectangles which in the *positive* discontinuous shape were in the same orientation as that of the original *negative* shape (test 6). The two squares (test 4) were included to discover whether in this situation the bottom halves of shapes are more important in discrimination

than the top halves. If the bottom halves are more important, rats trained to run to a vertical shape should choose the lower square, whereas rats trained to run to the horizontal shape should choose the upper square. If the top halves are more important, choices should be the reverse of this. In fact animals tended to treat the lower square as a verticle rectangle and the upper square as a horizontal, though

FIGURE 2
RESULTS OF TRANSFER TESTS

	Shapes	No. of trials per rat	Per cent "correct" responses
	 	80	95
1	 	10	91
2	 	10	79
3	 	10	87
4	 	26	70
5	 	12	86
6	 	12	74

The first pair of shapes represents the original shapes; the remaining six pairs are those used in transfer tests. Shapes are drawn to scale and the dots represent the position of the milk nozzles. A "correct" response was counted as a run to the transfer shape shown on the left by rats trained with the vertical positive, and to the right-hand transfer shape by animals trained with horizontal positive.

transfer was far from complete. That rats had not learned the discrimination *merely* in terms of relative height of the lower parts of the shapes is, however, shown by tests (3) and (5), since in (3) there is only a small difference between the height of the lower edge of the shapes and in (5) there is no difference.

The results on broken figures confirm Lashley (1938) and the greater importance of the lower halves of shapes is in line with the results of Munn (1930).

TRANSFER TO SINGLE SHAPES

Animals were also given transfer tests in which one of the original shapes only was displayed: on the other side the drinking nozzle alone protruded from the rear wall. Each rat was given 12 trials with the original positive shape on its own, and 12 trials with the original negative shape on its own. Animals ran to the positive shape on 76 per cent. of trials; they selected the opposite side to the negative shape on 74 per cent. of trials. This demonstrates that animals had learned almost equally to select the positive shape and to avoid the negative shape. Thus they had learned to some extent to react separately to the two component stimuli, although the possibility of some configurational learning having occurred cannot be ruled out. Fields (1932) using a Lashley jumping stand found no evidence of transfer when the negative shape was presented without the positive shape.

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REFERENCES

- FIELDS, P. E. (1932). Studies in concept formation. *Comp. Psychol. Monogr.*, 9 (No. 2), 1-70.
- GELLERMAN, L. W. (1933). Chance orders of alternating stimuli in visual discrimination. *J. genet. Psychol.*, 42, 206-8.
- HARLOW, H. F., and WARREN, J. M. (1952). Formation and transfer of discrimination learning sets. *J. comp. physiol. Psychol.*, 45, 482-9.
- LASHLEY, K. S. (1938). The mechanism of vision: XV. Preliminary studies of the rat's capacity for detail vision. *J. gen. Psychol.*, 18, 123-93.
- MCLEARN, G. E., and HARLOW, H. F. (1954). The effect of spatial contiguity on discrimination learning by rhesus monkeys. *J. comp. physiol. Psychol.*, 47, 391-4.
- MUNN, N. L. (1930). A note on Lashley's method for studying vision in the rat. *J. genet. Psychol.*, 37, 528-30.
- MURPHY, J. V., and MILLER, R. E. (1955). The effect of spatial contiguity of cue and reward on the object-quality learning of rhesus monkeys. *J. comp. physiol. Psychol.*, 48, 21-31.
- MURPHY, J. V., and MILLER, R. E. (1958). Effect of the spatial relationship between cue, reward and response in simple discrimination learning. *J. exp. Psychol.*, 56, 26-31.
- WEINSTEIN, B. (1941). Matching-from-sample by rhesus monkeys and by children. *J. comp. physiol. Psychol.*, 31, 195-213.

LATERALITY AND DIRECTIONAL PREFERENCES IN A SIMPLE PERCEPTUAL-MOTOR TASK

BY

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The superiority of tensor or outward movements of hand and arm over flexor or inward movements has been described, but no evidence has been found relating to directional preferences in simple perceptual-motor tasks.

One hundred children, aged 9 to 13, 50 being right-handed writers and 50 left-handed writers, were tested on a stroke-making task, using both preferred and non-preferred hands. 75 per cent. of them exhibited a preference for outward movements of both hands, a finding which applied equally to both handedness groups. Thus an explanation based on writing habits is discounted.

INTRODUCTION

It is generally accepted that tensor movements outwards from the body are superior to flexor or inward movements. The former have been described by several authorities as being smoother, more rapid, more accurate and less fatiguing (Bartlett (1957), Brown *et al.* (1948), Downey (1932)). Now it might well be suggested that this could be due not to neuro-muscular or central characteristics but to training and usage associated with left to right handwriting. This directional tendency might be transferred to other motor tasks as far as the right-hander's preferred hand is concerned, whilst his left hand might "mirror" the movement of the dominant one (cf. Van Riper (1935)). If this were so the subject who consistently writes with his left hand might be expected to display superiority of *inward* or flexor movements.

It may be presumed that the directions of movement *preferred* by subjects in the performance of simple tasks will be those which are more efficient and less tiring, but there is a lack of direct experimental evidence in support of this. Relevant data for the perceptually complex task of pursuit tracking have been presented by Grant and Kaestner (1955), with results which, at first sight, appear to contradict this presumption that preferred movements are in the outward direction. However, if a bias towards outward movements does in fact exist it seems reasonable to assume that it will be modifiable by the perceptual and manipulative components of any particular task.

The present study is aimed at finding whether subjects do in practice display any consistency of directional preference in a task requiring a minimum of perceptual response, and at determining whether this applies to both right- and left-handed writers.

METHOD

The method was identical with that used by the writers as part of a wider investigation (Smith and Reed, 1959).

Subjects. Fifty children who wrote with their right hands and 50 who wrote with their left were drawn from five schools. Ages ranged from 9 to 13, and there were equal numbers of boys and girls. Handedness groups were equated for sex, age and school.

Procedure. Subjects were required to "fill in the rungs of a ladder" by pencilling short horizontal strokes between two vertical lines 8 mm. apart. This was done with each hand (the order being randomized between subjects) for 45 sec. The task was presented as a speed test, no mention of stroke direction being made. In order to introduce a

competitive element (again stressing speed and thus encouraging spontaneity of movement) children were tested in small groups. They were, however, seated at 2 yd. intervals, and carefully supervised, to preclude imitation.

Preferences were established by direct observation of the directions in which strokes were made.

RESULTS AND DISCUSSION

Directional preferences proved to be quite consistent within each hand performance and in this sample no child was observed to vary the direction of his strokes to any appreciable extent. No significant differences were noted between sexes or ages. The data are therefore presented in one table. This shows subjects tabulated according to the directions preferred by both hands. In each pair of arrows the one on the left shows the direction preferred by the left hand, and that on the right shows the direction preferred by the right hand.

TABLE I
DISTRIBUTION OF SUBJECTS BY DIRECTIONAL PREFERENCES OF BOTH HANDS

Handedness Groups				1 ← →	2 → →	3 ← ←	4 → ←	
Right	..	..	..	35	13	2	—	50
Left	..	..	..	40	4	3	3	50
Totals	..	..	..	75	17	5	3	100

It will be seen that over the whole group 91 per cent. of subjects made tensor movements of the preferred hands, and 81 per cent. made tensor movements of the non-preferred hands. 75 per cent. of subjects, those figuring in column 1 of the Table, made tensor movements with both hands. Thus these results offer support for the view that in a simple motor task there is a general preference to make movements in an outward direction.

Grant and Kaestner's left-handed subjects expressed preference for a left-to-right target direction, whilst those right-handed subjects who were experienced in the tracking task preferred a right-to-left target direction (i.e. both groups preferred *inward* target directions). This finding, however, may be reconcilable with ours, for it appears that the apparatus used by these investigators necessitated pushing as opposed to pulling movements of the rotor handle in association with the directions preferred. Grant and Kaestner discounted perceptual factors as being responsible for the difference in preference and proposed a motor hypothesis related to an observation of Brown, Knauff and Rosenbaum (1948) that "positioning movements are more accurately made with a thrusting or extending motion of the hand and arm than by pulling or flexing motions." This would apparently reconcile Grant and Kaestner's results with those presented here for it would seem likely that "extending motions" are equivalent to what we have called "tensor movements." But Bartlett (1957) has made it clear that the superiority of such movements occurs when they are made horizontally outward from the centre of the body, a distinction supported by Downey's (1932) application to them of the term "centrifugal" as opposed to "centripetal." We cannot, therefore, incline to the view suggested by Grant and Kaestner; evidently the question of directional preferences in perceptually complex tasks is still open both to terminological and experimental clarification.

It is noticeable that the preference for outward movements is shared by both our handedness groups. If columns 2, 3 and 4 are pooled $\chi^2 = 1.33$ which is well below any acceptable level of significance and thus leaves undisproved the null hypothesis that bilateral outward directional preference is not associated with handedness preference. Evidently the directional habits associated with writing skills have not carried over into this task as far as the majority of our left-handed writers is concerned. The motor hypothesis based on writing usage could be sustained only by a preponderance of left-handed subjects figuring in column 4.

The major difference between handedness groups is to be noted in column 2. A far greater proportion of the right-handed writers seem to prefer making strokes with both hands in a left-to-right direction. If columns 1, 3 and 4 are pooled $\chi^2 = 4.53$ (after application of Yates' correction for continuity) which is significant at the 0.05 level. A possible explanation for the difference in proportion here might be made in terms of relative dominance. It has been observed (Hildreth, 1949) that the population of right-handers includes more individuals of pronounced lateral dominance than does that of left-handers, and it might be that the right-handed subjects figuring in column 2 are representatives of such extreme "one-sidedness." It would be anticipated that this dominance would be reflected in greater functional differentiation between the hands on test. In this case the actual scores (in numbers of strokes) made by these subjects on our task do not afford confirmation of this kind and the problem must remain open for further investigation.

REFERENCES

- BARTLETT, Sir F. C. (1957). *The Mind at Work and Play*. London.
- BROWN, J. S., KNAUFT, E. B., and ROSENBAUM, G. (1948). The accuracy of positioning movements as a function of their direction and extent. *Amer. J. Psychol.*, **61**, 167-82.
- DOWNEY, J. E. (1932). Back-slanted writing and sinistral tendencies. *J. educ. Psychol.*, **23**, 277-86.
- GRANT, D. A., and KAESTNER, N. F. (1955). Constant velocity tracking as a function of subjects' handedness and the rate and direction of the target course. *J. exp. Psychol.*, **49**, 203-8.
- HILDRETH, G. (1949). The development and training of hand dominance. 1. *J. genet. Psychol.*, **75**, 197-220.
- SMITH, A. C., and REED, G. F. (1959). An experimental investigation of the relative speeds of Left- and Right-Handed Writers. *J. genet. Psychol.*, **94**, 67-76.
- VAN RIPER, C. (1935). The quantitative measurement of laterality. *J. exp. Psychol.*, **18**, 372-82.

BOOK REVIEWS

Readings in Social Psychology. Edited by E. E. Maccoby, T. M. Newcomb and E. L. Hartley. 3rd edition. London. Methuen. 1959. Pp xi + 674. 55s.

In 1943 the Society for the Psychological Study of Social Issues became worried by the lack of teaching materials that could "adequately portray the empirical foundation upon which social psychology rests." They appointed a committee which, after considering the matter, recommended that "a volume of readings should be prepared which should be . . . representative of the reports of research in social psychology and of the methods by which its conclusions are reached . . . [and] that the interdisciplinary nature of the field be stressed." In 1947, the first edition of "Newcomb and Hartley" made its bow. Its success was such that in 1952 a second edition appeared. Now we have the third—three editions in 11 years. Speaking as a teacher of social psychology, I am not at all grateful. If you buy it (55s.) and already have the second edition, you are buying a book you already half possess—one-third, if you have the first. Every time a new edition appears, all the readings are shuffled around and reclassified under newly devised headings. Giving references to it is a nightmare.

The readings, say the editors, are illustrative but do not aim at being representative. "Usefulness" to the teacher is the criterion they put their faith in, but they do not say how they determine this. The topics covered, however, are the central topics of many textbooks and we may guess they are in fact representative of the major theoretical interests of pace-setting social psychologists in America today. The editions may therefore give us some indication of what these are and how they have changed.

Common to all editions are the "classics." The Hawthorn investigation, Lewin's work on group decision, Lippitt and White's study of leadership, the California Authoritarian Personality study and so on, 18 of them altogether, more surely than merit this status. The difference between the two editions—the second occupies an intermediate position—is mainly a matter of scale and method. The scale has dwindled or the methods become more quantitative. Anthropological and sociological studies of the natural history kind play a far smaller part. This is particularly noticeable in the sections on role, language and leadership. Perhaps this has something to do with the very volatile composition of the editorial committee responsible in each edition for making the selections.

But there has also been some change in fashion. The Culture Pattern and Personality thesis, with its associated emphasis on child-rearing methods, social class and the process of socialization, all pretty macroscopic phenomena entailing greater permissiveness in research-methods, accounted for many items in the first edition. An interesting article by Bronfenbrenner in the present edition suggests a relation between methods of upbringing and differential class-sensitivity to pediatric fashion, on the basis of a survey over time of studies of class differences. But it is unlikely that the Chicago sociologists will be quite so easily written off.

Two main emphases mark the new edition. The first of these is on cognitive aspects of social psychology. These include some experimental work on the relation between language and thought; a section on "Perception of Persons," which pays tribute to the growing work on this question; and in the field of communication and opinion change, three contributions from the Institute of Human Relations at Yale which testify to the excellence of the work done under Professor Hovland's leadership there, though it could be argued on the "usefulness" criterion, that their report in a separate book renders unnecessary their inclusion among these readings.

The second main emphasis is on interaction in small groups. This is very pervasive, entering into about 40 per cent. of the contributions. Even the topic headings, not immediately concerned with groups, include reports where such processes as perception of persons, role behaviour and race prejudice are studied within a small group setting. The greatest amount of experimental *finesse* is shown here and the greatest scale contrast with the first edition. Of the investigations reported we would only mention that of Pelz, who shows *contra* Lewin that if a group decision is made, not necessarily for that reason is it kept.

The editors say they have carried "even further the empirical principle adopted in the two previous editions." This has been at the cost of "theoretical writing" for "while

teachers of social psychology may differ widely in their theoretical approach . . . they share the need for reports of sound empirical research which they can offer to their students as a foundation for theoretical thinking." But according to their theoretical outlook, teachers will differ in the facts they consider important, indeed in the extent to which they think *just facts* important at all. It may be worth considering certain weaknesses these selections suggest in the light of this emphasis and the much reduced representation of large scale descriptive work. One striking feature is the artificiality of some of the situations studied. Something which is easily observed or manipulated is given a sociological name so as to make the investigation significant. Serial reproduction is called rumour; a "group" is often two people, and a "cohesive group," two people who have just met to do some task together with the information that the other partner is desirable; a most careful study, showing among other things that children rated as leaders influence others more frequently both directly and by suggestion, is labelled "The Dynamics of Power" and Max Weber referred to. Much more satisfactory are the *few* investigations where advantage was taken of certain naturally occurring or administratively planned phenomena, as in Festinger, Riecken and Schachter's interesting study of chiasm or Deutsch and Collins' study of the effects of inter-racial housing policy on race prejudice. A second weakness is insufficient precision in some of the terms used. "Role," a technical term, linked as the first edition made clear with the concept of status, has undergone contamination from ordinary usage and is now also used to refer to the part anyone may choose to play in, say, an informal group trying to solve a problem. Similarly, "leadership" includes both those who, within a group of peers, emerge to play a leading part, and adults who conduct groups of small boys in carrying out some prescribed task. Contrasting with the experimental discipline, there is a lack of discipline in the analysis of terms, in "theoretical thinking," a feature which often makes it difficult to compare investigations carried out ostensibly on the same problems.

Finally, one is struck by the great elasticity of the term "social," as applied to psychology. Among topics dealt with are language, politics, industrial management, propaganda, religion, brain-washing, conversation, initiation ceremonies in primitive societies, rumour, class, race prejudice and drug prescription by doctors. They range from microscopic, social physics type of investigations, such as those on person-perception to studies relating to Marxist class interest theory. I do not think it is just the way these readings have been selected that gives the impression of a sort of trigger-happy experimentalism, and anyhow the editors bear some responsibility for the image of social psychology they present to each new generation of students. Apart from certain areas where society's needs have led to fairly intensive cultivation, the impression we get is one of tiny sophisticated colonies of town-dwellers, dots on a wide plain with poor communications between them because they have not thoroughly explored, let alone surveyed, the surrounding country.

What one would like best would be a volume of readings with material chosen according to three principles—detailed material from one area of *applied* social psychology; readings chosen to illustrate different methods, including those of sociology and social anthropology; and a compact body of material, closely integrated in terms of some reasonably articulated theory. And non-overlapping supplements every 5 years. But I cannot persuade myself that future editions will take any notice of such high demands. Perhaps after all, one should be more modest and ask only that the readings, please, be consecutively numbered and the discrepancies between figure and text on page 23 corrected.

W. H. N. HOTOFF.

Physiology of the Retina and the Visual Pathway. By G. S. Brindley. (Monographs of the Physiological Society, No. 6.) London. Edward Arnold (Publishers) Ltd. 1960. Pp. xi + 298. 35s.

This is indeed a tightly written book and full of information, as the author's introduction says. The references are admirable. Even on those aspects which he treats most high-handedly the most important references are sure to be found. On the photochemistry of the retina, its electrical activity and its interpretation, he provides an account at once critical and comprehensible. Parts of this chapter could with advantage have been expanded. The account of lateral inhibition is not really intelligible without a diagram, or without looking up the original papers of Kuffler and of Barlow, FitzHugh and Kuffler. The confusion which has been introduced by studying the effect of large field illumination on single units might also have been the subject of critical comment. If the Baltimore workers are right that in the cat there are only "on" and "off" receptors,

and that "on-off" effects are due to illuminating the receptive field and its inhibitory surround simultaneously, then it seems idle to describe the varieties of response to large field illumination.

In the chapter on central connections, anatomical considerations receive rather limited attention, and the carping critic notes that Minkowski's paper in the *Arbeiten aus Hirn-anatomische Institut, Zurich*, which is not very difficult to obtain and gives the classical account of geniculo-cortical connections in the cat, appears only as cited by Le Gros Clark. Henschen appears in the list but briefly, considering his 40 year struggle to establish the nature of central representation, and, though Gordon Holmes appears, no reference is made to his collected papers which include his most important contributions on patients with verified lesions. The reviewer also feels that Spalding's careful studies in 1939-45 have advanced the subject. This whole subject is, of course, treated at such length in Polyak's posthumous book that a rather perfunctory treatment here is not of great importance.

The real strength of the book lies in the second half on sensory experiments and their division into Class A, those in which two sensory stimuli are compared, and Class B, those in which the subject must describe the quality and intensity of his sensations. On these, the author is rigorous and encyclopaedic. The reviewer is aware of his incapacity to criticize and can only admire the clarity of the chapter on the visual threshold.

This is a book for research workers, and only for the benefit of the ignorant reader, and it is assumed that the serious reader will be prepared to pursue most topics to the original papers. While this is both admirable and salutary, one feels that even research workers would benefit from a little more high-lighting of major conclusions and omission or compression of the results of the less successful techniques.

It is by far the most learned, critical and penetrating of the monographs so far issued by the Physiological Society, and sets new standards which others will find very difficult to attain.

D. WHITTERIDGE.

Cerebral Dominance and its Relation to Psychological Function. By O. L. Zangwill. Edinburgh. Oliver & Boyd. 1960. Pp. vii + 31. 10s. 6d.

It is interesting to note that the substance of this book was delivered as lectures under the auspices of the Henderson Trust, set up by Mr. William Ramsay Henderson who died in 1832, leaving a sum of money to be devoted to "the advancement and diffusion of the Science of Phrenology." Although we have come some distance since that date, we are still in considerable doubt as to the inter-relation of function of the two cerebral hemispheres, particularly with respect to language and to laterality. Professor Zangwill has collected some valuable evidence from cases of unilateral brain injury which indicates that only in fully right-sided individuals is there a clear dominance of one hemisphere over the other. Other persons show varying degrees of sinistrality; and their language functions may not be completely specialized in either hemisphere.

It seems also that this lack of clearly established dominance may be associated with inadequacy in the language functions and in motor control; and that the condition may be genetically determined. Evidence on this was obtained by Professor Zangwill from a study of children referred to a clinic for their inability to learn to read normally; and it is hypothesized that in many of these cases who showed mixed laterality the disability was associated with failure of cerebral dominance to develop normally. Interestingly, some of these cases also showed an inability to copy simple perspective figures, and a deficiency in spatial concepts such as that demonstrated by Professor Zangwill in cases of injury to the parietal lobe of the minor hemisphere.

It has also been found by other psychologists that defects of this kind are often associated with temperamental instability; and Professor Zangwill suggests that these individuals may be unduly sensitive to "stress." The reading disability may then be a function of these factors as much as of any congenital disposition towards failure in the normal development of cerebral dominance. We must conclude that the condition is as yet too ill-defined and diffuse to justify us in labelling it "specific dyslexia," with the implication that it is a unitary condition, and that lack of cerebral dominance is the most frequent and fundamental cause of reading disability. There may be evidence of this condition in many clinic cases, and perhaps it is the cause of the most severe and intractable reading disability; but on this much further evidence is required. Meanwhile, all who are interested in the fascinating problems relating to the language functions of the cerebral cortex will welcome this study of Professor Zangwill's.

M. D. VERNON.

Principles of Comparative Psychology. Edited by R. H. Waters, D. A. Rethlingshafer and W. E. Caldwell. London. McGraw-Hill. 1960. Pp. ix + 453. 62s. net.

The editors of this introductory textbook have had the "broad objective . . . to create a book in comparative psychology rather than in animal psychology," and to "incorporate recent findings" in such areas as the effect of early experience, innate behaviour, neuropsychology, behaviour genetics and ethology. Such an elementary text would indeed be welcome in view of the fact that Maier and Schneirla's classic, *Principles of Animal Psychology* is out of date and Thorpe's *Learning and Instinct in Animals* is perhaps rather advanced for the elementary student. However, although the aims of the editors of this new book are commendable, their procedure and results leave something to be desired. *Principles of Comparative Psychology* consists of a series of chapters by different authors. Not only do the level and accuracy of the different chapters vary considerably, but also there is less cohesiveness, more overlap and poorer cross-referencing than one would hope for in such a primer. Furthermore, it seems to me, that this book has largely failed in its aims to be "comparative" and to incorporate recent developments in ethology and neuropsychology.

Data and theory are drawn principally from rats, monkeys and men. Ethology is still represented by the early and crude "hydraulic" and "neural" models of Lorenz and Tinbergen, whereas the recent critical ethological formulations and attempts at a rapprochement with learning theory such as those of W. H. Thorpe and R. A. Hinde are hardly mentioned. The dust jacket speaks with apparent pride of "A unique departure from recent textbooks in this field . . . the inclusion of a chapter on the problem of classification of behaviour in which zoological taxonomy is compared with what will some day become behavioural taxonomy." The authors of this chapter consistently confuse causal, functional, ontogenetic and phylogenetic classification, misrepresent the views of H. W. Nissen, one of the most creative workers in this field, and seem to conclude that zoological taxonomy is of little relevance to the student of behaviour. The chapter on innate behaviour is described as "a new look at the old problem." The "new look" turns out to be the startling insight that behaviour is a function of heredity, environment and previous experience and should be studied as such. Tinbergen's greylag goose and stickleback are mentioned and "ethological theory" is mildly chastised. In the 22 pages allotted to "Behaviour and the Nervous System" the authors only have space to survey, on a fifth form level, the nervous systems of various phyla, to devote two pages to methods of analysis and virtually to list the types of behaviour which can be affected by manipulating the central nervous system. The omission from this chapter of a description of a single experiment and the failure to summarize the progress of analysis in a single area (e.g. hunger, sleep, emotion) seems rather unfortunate. The nervous structures and behaviour of Protozoa are ignored in this chapter as well as in the other portions of the book.

Indeed the best chapters by far are on the traditional interests of American rat and monkey psychology. The chapters on curiosity-investigative motives (R. A. Butler), learning (A. H. Riesen), complex processes, i.e. more difficult learning (A. J. Riopelle), abnormal behaviour (J. A. Dinsmoor), and genetics (J. L. Fuller) are all written with clarity and critical acumen and are flavoured with the theoretical orientation of the particular author. These stimulating essays are highly recommended to both the student and research worker in psychology.

Although the editors' ignorance of recent European developments may be understandable, the omission or scant treatment of the important American comparative work, such as that of Beach on sex, Griffin on animal navigation, Dethier and Schneirla on insects and Aronson and Lehrman on the role of hormones, is less so.

Dubious and vague statements are rather common, e.g. "The locus of drive for the higher animals is probably in the hypothalamus or related neural areas" (p. 104), "Instincts are released into behaviour under the influence of sign stimuli" (p. 127), "Trophallaxis is a complex relationship involving mutual care which is highly developed in the social insects" (p. 265).

Waters, Rethlingshafer and Caldwell's textbook is a reflection of the increasing interest of American psychologists in animal behaviour *per se* rather than in the use of laboratory animals to study learning or motivation and of their growing interest in the physiological bases of behaviour. Let us hope that the high standard of American science textbooks will soon extend to these areas, as it has to others in psychology.

CHARLES G. GROSS

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Part 3

OBSERVING RESPONSES AND DISCRIMINATION LEARNING

BY

H. PHILIP ZEIGLER¹ and L. B. WYCKOFF, Jr.²

From the Department of Psychology, University of Wisconsin

An operant response in the pigeon, whose performance results in exposure to the discriminative stimuli, is described and suggested as an experimental analogue for "observing." Such an operant response is then used to explore the relationship between observing responses and discrimination learning in a variety of discrimination situations, of progressively increasing complexity. In general, the results support the contention that the development and maintenance of observing responses is closely related to the degree of differential behaviour manifested toward the discriminative stimuli. Certain modifications are suggested in the theoretical formulation underlying the concept of "observing responses."

It has been suggested by certain theorists (Kelleher, 1958; Reid, 1953; Wyckoff, 1952) that discrimination learning may be conceived of as involving two distinct but related types of response. The first type includes responses upon which reinforcement is directly contingent, such as key pecking, lever pressing, turning right in a maze, etc. Such responses may be termed "effective responses" and distinguished from responses which, while not directly related to reinforcement, are instrumental in providing for exposure to the discriminative stimuli. Responses of this latter type have been variously labelled but in the present paper the term "observing responses" will be used.

The problem of observing responses may be approached experimentally in a number of ways. The most direct method involves the study of the actual orientation behaviour of subjects during discrimination learning, and, perhaps, the physiological concomitants of such behaviour. Thus it is possible to study directly head and eye movements and the electrical activity of the brain (EEG) and autonomic nervous system (GSR). Examples of this approach may be found in the work of Morrell and Jasper (1956) and Sokolov (1956).

Alternatively, observing responses may be treated as a theoretical construct inferred from differences in behaviour which result from differences in experimental conditions. This approach is best exemplified by Lawrence's studies on the "acquired distinctiveness of cues" (1949, 1950) and the studies of Reid (1953) and Pubols (1956) on the "response of discriminating."

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Such approaches have certain drawbacks. When the observing response is treated purely as a construct, the actual responses which may be involved are not subjected to observation and measurement. If, on the other hand, observing responses are equated with some limited and specific set of responses, such as orientation behaviour, the other responses involved in observing may be overlooked. Thus, the focusing and accommodation responses of the eyes are difficult to record and there may be still other internal responses which are even less accessible to measurement.

An alternative approach has been developed by Wyckoff (1951, 1952). After distinguishing between "effective" and "observing" responses in the discrimination situation, he points out that probability of reinforcement on any given trial of a problem is not contingent upon making an observing response, but solely upon the effective response. On the basis of a theoretical analysis of the problem of observing, he proposed the following criteria for an "observing response":—

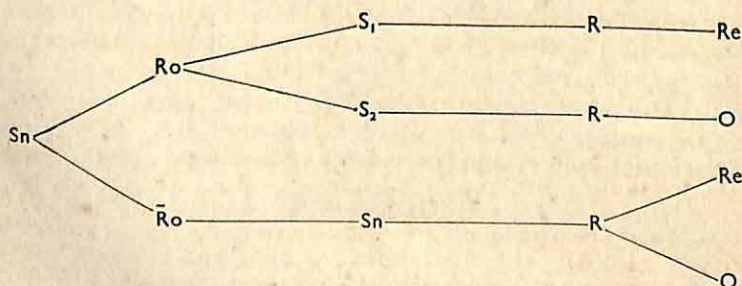
- (1) The subject is exposed to the discriminative stimuli if, and only if, the observing response is made.
- (2) The observing response need have no effect upon the probability of reinforcement on any given trial.

On the basis of these criteria, Wyckoff set up an experimental analogue for "observing" which involves a measureable operant response. Thus the major advantage of his approach is that "observing responses" may be treated as operant responses, amenable to observation and measurement.

Initially, the subject is faced with a set of neutral stimuli (S_n), such as a pair of blank response keys. Stepping on a pedal serves as the observing response (R_o) since it results in exposure to the discriminative stimuli (S_1 , S_2), e.g. red and green lights. The pedal response was chosen because it is easily recorded and is relatively independent of the effective response (R) of key pecking. Furthermore, since the stimuli are projected on the key and the effective response of key pecking is occurring at a high constant rate, one can be reasonably certain that simultaneously making the pedal response does expose the subject to the discriminative stimuli. Finally, the experimental situation is so arranged that reinforcement (Re) on any given trial is not contingent upon making an observing response but solely upon the effective response of key pecking. If the observing response is not made ($\bar{R}_o$), the subject continues to be exposed to neutral stimuli and the effective response to these stimuli is reinforced on 50 per cent. of the trials.

The experimental paradigm used in Wyckoff's experiments (1951) is diagrammed below:

FIGURE 1



Wyckoff hypothesized that "exposure to discriminative stimuli will have a reinforcing effect upon the observing response to the extent that the subject has learned

to respond differently to the discriminative stimuli." This hypothesis, and a number of subsidiary ones, were tested in a discrimination situation involving the use of the operant observing response. These experiments (Wyckoff, 1951) demonstrated that the gradual acquisition of observing responses closely parallels the formation of a discrimination. That is, under conditions of differential reinforcement of the discriminative stimuli the proportion of time on the pedal increases as the discrimination is learned, while it decreases and remains low under conditions of non-differential reinforcement. Furthermore, when a well established discrimination is reversed, the observing response decreases temporarily and then gradually returns to a high level as the new discrimination is learned.

Subsequently, a number of related experiments by Holland (1957), Reid (1953) and Kelleher (1958) have provided support for the contention that observing responses play an important rôle in the acquisition of discriminations. Furthermore, Holland (1957) and Kelleher (1958) have also shown that observing responses, like other operants, may be developed, maintained, and controlled by the application of reinforcement principles. These previous studies have involved only a single pair of relatively simple stimuli. However, both Wyckoff (1952) and Reid (1953) have envisaged the extension of observing response theory to more complex discrimination situations. In the present study therefore, the relationship between observing response development and discrimination learning is explored in three different discrimination situations of progressively increasing complexity.

In the first situation, a single simultaneous pattern discrimination is learned to criterion. It is hypothesized that the level of observing response development will be directly related to the level of discrimination performance. The second situation consists of the presentation of a series of such discriminations, each of which is learned to criterion. It is hypothesized that the introduction of each new problem will be accompanied by a temporary decrease in observing responses, but that these will eventually tend to attain a stable, relatively high value. Finally, in the third situation, the development of a discrimination learning-set is studied; i.e. each of a large number of problems is presented for a fixed and relatively small number of trials. In this situation, it is predicted that the level of observing response development will be directly related to the level of learning-set formation. In all three situations, an operant observing response of the type developed by Wyckoff is used to explore the relationships between observing responses and discrimination learning.

METHOD

Subjects

Sixteen pigeons (*Columba domestica*), obtained from a commercial supplier, were used. They were divided into two equal groups, half serving in the experimental and half in the control group.

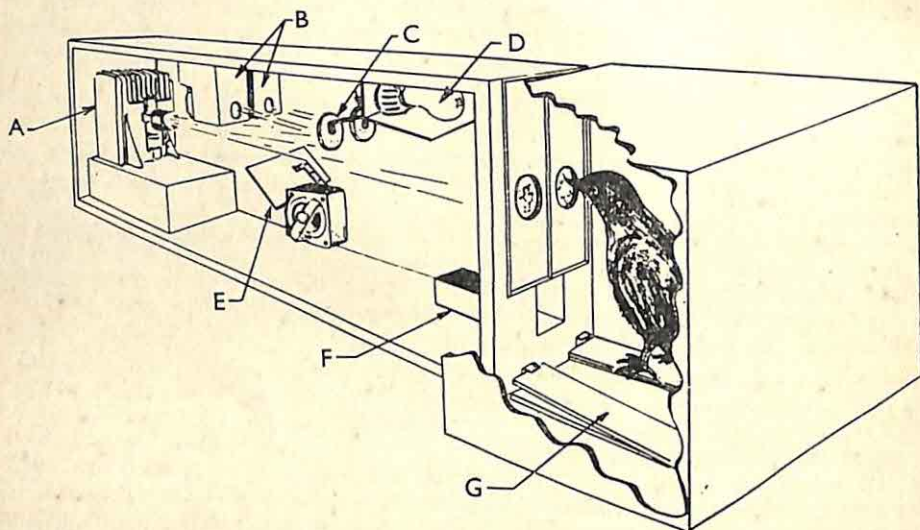
Stimuli

These were selected from a large assortment of randomly paired objects maintained by the University of Wisconsin Primate Laboratory. They were photographed in colour and projected upon the operant response keys by a 16 mm. strip-film projector. Each frame of film consisted of a pair of objects and constituted a trial. Film strips of 24 and 48 trial problems were used, with the position of the positive object in each pair being randomly varied according to a prearranged Gellerman sequence. The position of the positive stimulus in each frame was coded by photographing it in front of a black patch, placed so as to be invisible to the subject, but coincident with the photocell which provided signals for the reinforcement mechanism.

Apparatus

A diagrammatic representation of the apparatus is presented in Figure 1. A square wooden box of $\frac{1}{4}$ in. plywood with a translucent top served as the subject's compartment. Its front was open and gave access to a second compartment containing the response keys and food magazine. The response keys were of translucent plastic and a small square of draftsman's tracing paper affixed to their backs served as screens for the projected stimuli. Each key was hinged and mounted in combination with a pair of leaf contacts so that when they were struck lightly an electrical circuit was completed. The food magazine (F) contained grain and was opened for 6 sec. automatically whenever a correct response was made.

FIGURE 1



Diagrammatic representation of the apparatus.

- A. Strip-film projector. B. Photocells. C. Mirrors. D. 60-watt lamp. E. Shutter.
F. Food magazine. G. Observing response pedals.

The observing response pedals (G), constructed of sheet metal 10 in. long by $2\frac{1}{4}$ in. wide, extended $\frac{1}{2}$ in. above the floor of the subject's compartment. They were so placed with respect to the keys that while simultaneous key and pedal responses were possible, responses to either or both keys could be made without stepping on the pedals.

The second compartment also contained the strip-film projector (A), two photocells (B) and a shutter mechanism (E). A portion of the projector beam was deflected into the photocells by means of appropriately placed mirrors (C). These photocells provided the signals for the film advance and reinforcement mechanisms, utilizing information coded on the film. The shutter was so located that when it was activated, the projector beam was completely blocked, leaving the keys in darkness. A 60-watt frosted lamp (D) was mounted above and behind the keys.

Experimental procedures were completely automatized and operating and recording circuitry were located in an adjoining room to eliminate auditory cues. Two printing counters cumulated positive and negative key pecking responses. The observing response pedals were connected to a vacuum-tube pulse generator, which produced 10 pulses per sec. whenever either of the pedals was activated. This output was fed to a third cumulative printing counter and recorded duration of pedal pressing for each trial.

Procedures

Pretraining. Subjects were adapted to the apparatus, after being placed on a deprivation schedule, and trained to make the effective response of key pecking. Intermittent reinforcement was then introduced, key pecking being reinforced on a 40 sec. Fixed

Interval schedule. No training or reinforcement of any kind was given with respect to the pedals, though they were present in the compartment during this period.

General procedure. One experimental session of 48 min. duration was given daily. Each session consisted either of one 48 or two 24 trial problems. Each trial lasted 40 sec. and consisted of the presentation of a single frame of film containing the stimulus pair. During this time, key pecking responses were recorded. The first response following this interval of 40 sec., if made to the positive stimulus, was reinforced, i.e. the food magazine was opened for 6 sec. If the response was made to the negative stimulus, reinforcement was withheld. In either case, the shutter was simultaneously activated, the film advanced one frame and a 20-sec. rest interval ensued during which time the keys were unlighted.

For the control group, the observing response pedals were present in the compartment so that a record could be kept of time on pedal. However, the stimuli were present throughout the trial and pedal pressing had no effect upon presentation of the stimuli. For the experimental group, however, the pedals were connected to the shutter in such a manner that the shutter dropped and exposed the stimuli only during the actual intervals that either one of the pedals was being depressed. If a pedal was not depressed, the shutter remained interposed between the projector beam and the keys, and the only illumination on the keys was the uniform white light provided by the 60-watt lamp (D). (This lamp was not switched on at all for the control group, but was switched on for the experimental group until they pressed a pedal, when it was replaced by the stimuli to be discriminated.) This insured that during the trial general illumination of the keys was always of comparable intensity for both groups.

The reinforcement schedule was programmed directly on the film and the position of the correct stimulus randomly varied, so that if observing responses were not made, responses to the illuminated keys had a 50 per cent. probability of reinforcement on any trial. Thus it was not necessary for the observing response to be made in order for subjects to be reinforced for the effective response of key pecking.

Before starting learning-set training, subjects were given six criterion problems. Each problem was presented for 48 trials per day and run to a criterion of 75 per cent. correct responses in the last 12 trials of a given daily session. When this criterion was met, the next problem was presented at the next session. Learning-set training was begun after the subject had solved all six criterion problems. Two 24 trial problems were presented at each session and subjects received a minimum of 120 such problems.

In general, the only food provided was that obtained as reinforcement during the test sessions. However, occasional supplementary feedings were given to maintain the subjects at approximately 80 per cent. of their original *ad lib.* feeding weight.

Experimental design

A. Control Group LS ($N = 8$). Following criterion training, subjects were presented with 120 learning-set problems. There was no contingency between pedal pressing and exposure of the stimuli.

B. Experimental group. The subdivisions are *post-facto*, having been made on the basis of the results obtained, for purposes of analysis, rather than selected at the beginning.

(1) Group Ro I ($N = 4$) was made up of subjects who failed to solve all six criterion problems but solved one or more.

(2) Group Ro II ($N = 4$) included those subjects who had solved all six criterion problems.

(3) Group Ro III ($N = 3$) consisted of all subjects who completed learning set training.

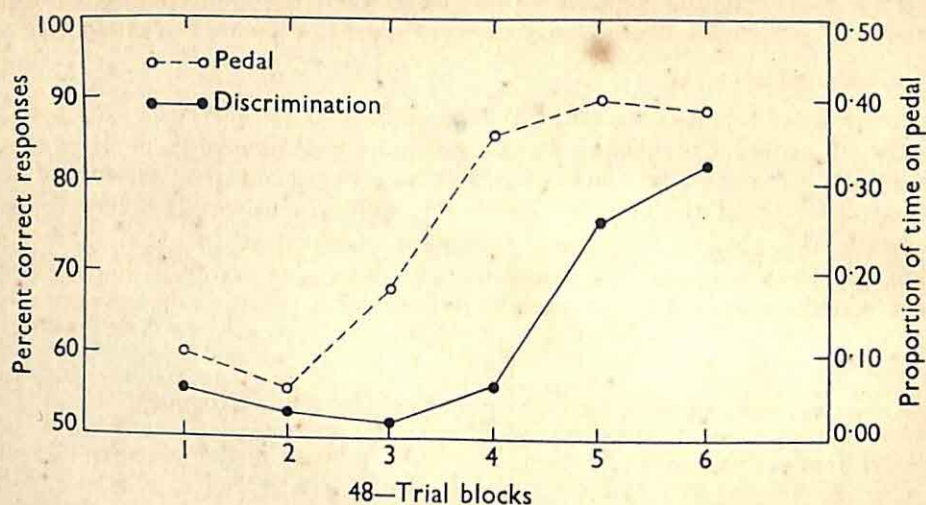
(4) Group LS/Ro ($N = 3$) was made up of subjects from the control group who were given additional learning set training under experimental conditions.

RESULTS

A. The single problem situation

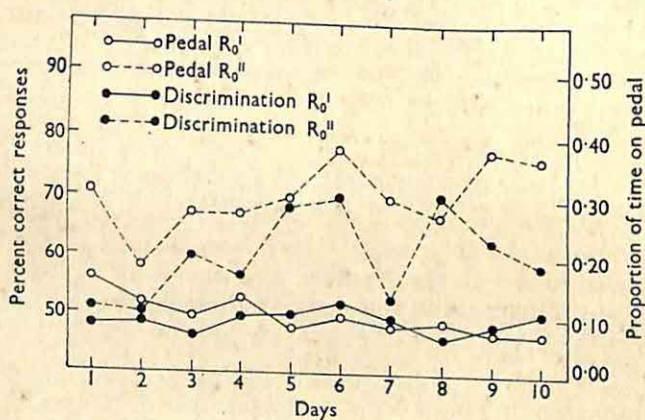
Figure 2 presents the learning and observing response curves for a single representative experimental subject on the first criterion problem. It indicates graphically the close relationship between discrimination learning and observing response development in a single problem learned to criterion.

FIGURE 2



Discrimination learning and observing response acquisition for a single subject on the first criterion problem.

FIGURE 3



A comparison of observing response development in subjects manifesting a high degree of discrimination learning and subjects in whom the degree of discrimination learning is low.

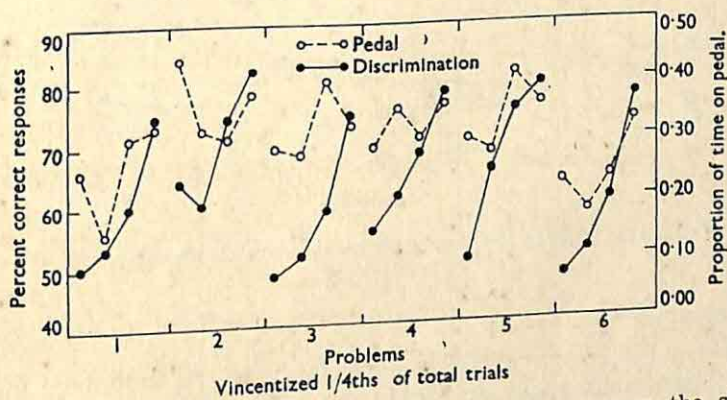
Figure 3 presents a comparison between the observing response development of subjects manifesting a low degree of discrimination performance and that of subjects in whom the degree of discrimination is relatively high. (The sharp drop in the discrimination curve of Group Ro II is due to the introduction of a new problem, some of the subjects in this group having learned the first problem by the sixth day.) Analysis of variance indicates that the proportion of time on pedal is significantly higher ($p < 0.01$) in Group Ro II, the group showing a relatively high level of discrimination. Conversely, in Group Ro I, proportion of time on pedal remains low and tends to decrease, while it tends to increase in Group Ro II concomitantly with increases in the degree of discrimination.

Subjects in Group Ro I were run for as long as 15 to 25 days on a single problem without attaining criterion solution. In all cases where discrimination was low and remained low, proportion of time on pedal continued to decrease or remain low.

B. Criterion problems

Figure 4 shows the discrimination and observing response curves for Group Ro II across the six criterion problems. Because of individual differences in trials to criterion, results are plotted in Vincentized fourths. In general, the data support the hypothesis that the introduction of each new problem is usually followed by a temporary decrease in observing responses. Unfortunately, the number of criterion problems presented is insufficient to determine whether the observing response is tending toward a stable relatively high level.

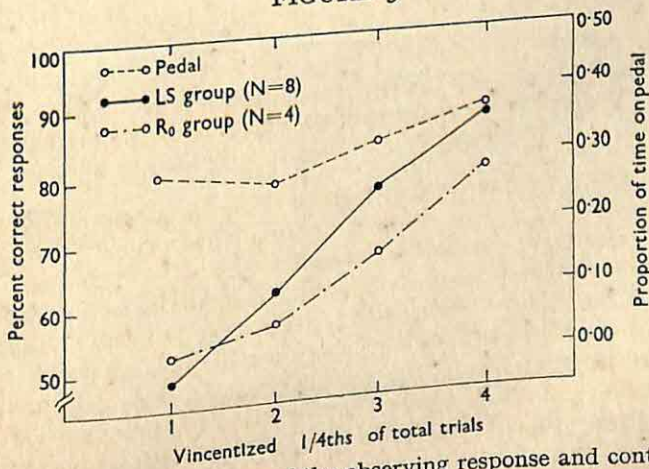
FIGURE 4



Discrimination learning and observing response acquisition across the six criterion problems.

Solution of the criterion problems took, on the average, significantly longer for the observing response group ($p < 0.05$). However, Grant's (1956) extended test for trend, when applied to the intraproblem learning curves of the two groups (Fig. 5), indicates no significant differences in the shape of these curves.

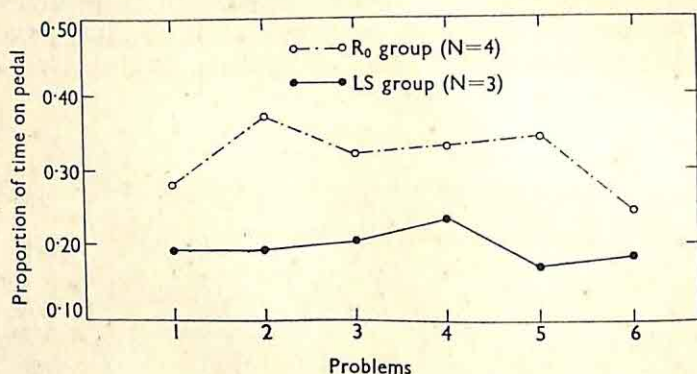
FIGURE 5



Comparison of the learning curves of the observing response and control groups.

In the control group, exposure to the discriminative stimuli is not contingent upon the observing response and we would therefore predict a relatively low level of observing responses by contrast with the experimental group. The data from three control subjects for whom pedal-pressing data was available is compared with that of Group Ro II in Figure 6. Analysis of variance indicates that between group differences in pedal pressing are significant ($p < 0.05$).

FIGURE 6

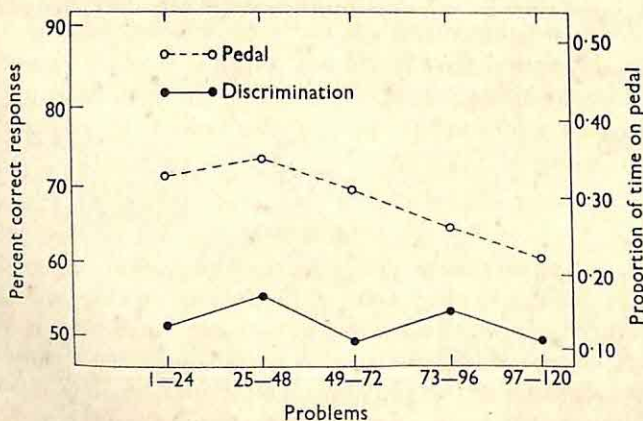


Comparison of pedal pressing in the experimental and control groups across the six criterion problems.

C. Learning-set formation

Only three subjects were available for inclusion in this group and none showed any signs of statistically significant learning set within 120 problems. At the outset, the level of observing responses was approximately the same for all three. However, Subjects No. 6 and No. 12 tended increasingly to respond in terms of position preferences, and, as can be seen from Figure 7, proportion of time on pedal was steadily decreasing. Subject No. 13, however, was beginning to show improved performance both on the discrimination problems and in the use of the observing response. That is, not only was he responding more consistently to the stimuli, rather than to position,

FIGURE 7

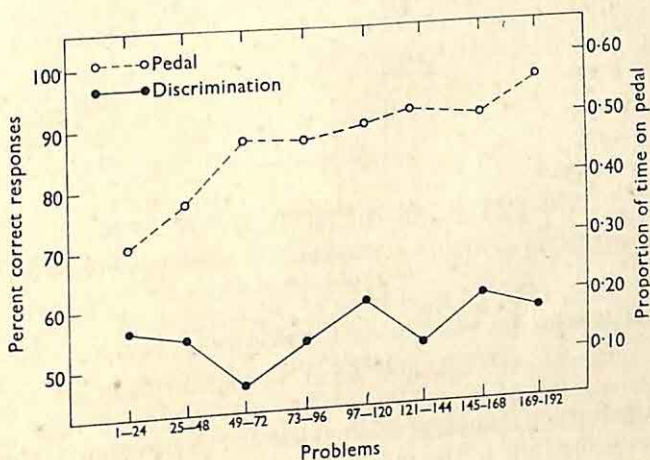


Learning-set formation and observing response development in subjects No. 6 and No. 12.

but he appeared to be learning to use the pedal response to provide exposure to the stimuli. (An incidental observation which was repeatedly confirmed, is of interest here. If the subject was pecking at a key which was lighted white, and then stepped on the pedal and exposed the incorrect stimulus of a given problem, he would often get off the pedal, walk to the other side, stand on that pedal, expose the correct stimulus and begin to respond to it.)

For these reasons, learning-set training was continued for Subject No. 13 until it was clear that the subject's discrimination performance was significantly better than chance expectation ($p < 0.01$). Under these conditions, proportion of time on pedal continued to increase. It should be noted that such increases were evident long before there was any significant improvement in discrimination learning (Fig. 8).

FIGURE 8



Learning-set formation and observing response development in subject No. 13.

D. The LS/Ro group

This was made up of three control subjects who had attained a significant degree of learning-set formation and were then given additional training under the observing response condition. (Subject No. 3 rapidly developed a position habit and training was discontinued. Subjects No. 5 and No. 9 had a high operant level of pedal pressing prior to their being placed in the experimental condition.) As Figure 9 indicates, the LS/Ro group not only did not continue to show increasing learning-set formation, but instead showed a marked decline in such learning. Furthermore, there is no consistent relationship apparent between observing responses and discrimination performance.

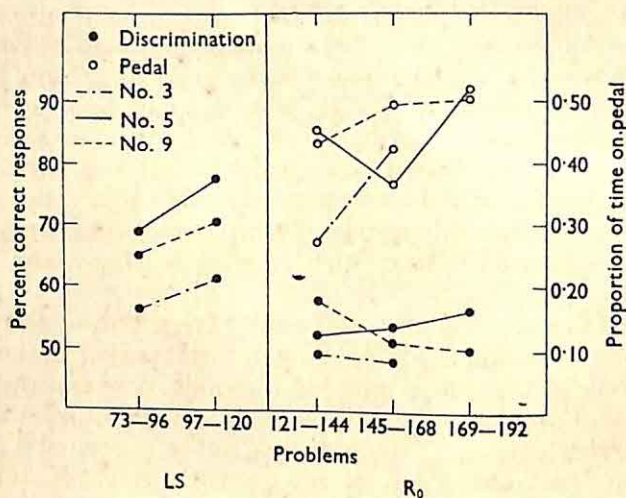
DISCUSSION

The present experiments were designed to apply observing response theory to learning situations in which more than a single pair of discriminative stimuli are involved. In addition, it was also possible to repeat and extend Wyckoff's (1951) original observations on the relationship between observing response development and intra-problem learning in the single problem situation.

The previous finding that observing response development closely parallels intra-problem discrimination learning was confirmed. Furthermore, these results were

obtained in a simultaneous discrimination situation with complex stimuli, while the original observations were made with a hue discrimination in a successive discrimination situation. The present results, therefore, extend the generality of the observing response concept to the two most commonly used discrimination situations.

FIGURE 9



Learning-set formation in Group LS/Ro before and after transfer to the observing response conditions.

In the situation where a single problem is learned to criterion, a direct relationship between degree of discrimination and level of observing response development was demonstrated. Furthermore, a similar relationship appears to obtain for each of a series of such problems. The introduction of each new problem is generally accompanied by a temporary decrease in observing responses, related to the low degree of discrimination at the outset of each problem. In such a situation, the observing response is being successively extinguished and reconditioned, i.e. intermittently reinforced. Unfortunately, the number of criterion problems presented appears to be insufficient to test the hypothesis that under these conditions the observing response will tend eventually to attain a high constant value. Similarly, in view of the small number of subjects involved and their failure to demonstrate any marked degree of learning-set formation, no conclusions may be drawn concerning the relationship between observing response development and learning set formation.

Certain more general findings remain to be discussed. Solution of the criterion problems took, on the average, twice as long for the experimental as for the control group, despite the fact that the learning curves for the two groups proved not to differ significantly. This is easily explicable if we assume that learning occurs only in the presence of the discriminative stimuli and recall that the stimuli are exposed to the experimental group only during that portion of each trial in which they are pedal pressing.

It was also found that half the experimental subjects did not solve all the criterion problems, despite the extensive training given, and that in all such cases, the level of observing responses was low and decreasing. Now it is readily apparent that there is a circular relationship between the level of observing response development and the

degree of discrimination. Thus, in the early trials of a given problem, both degree of discrimination and level of observing are low. Consequently, the subject is exposed to the discriminative stimuli for only a small portion of each trial and the degree of discrimination cannot increase rapidly. Conversely, the level of observing responses cannot increase rapidly, because of the low degree of discrimination. Thus, should the level of observing responses fall below a certain point it is likely to remain low or decrease as a result of what may be metaphorically termed "positive feedback" processes. The data presented in Figure 3 suggest that such processes, once initiated, are not spontaneously reversible under the conditions of the present experiment.

There is some evidence that at this stage of the problem, activity level may be a relevant factor. Initially, Group Ro II (Fig. 3) shows a high level of pedal pressing—possibly related to activity consequent upon their introduction into a novel situation—followed by a sharp drop and subsequent recovery. Both this initial level of pedal pressing and the baseline to which it recovers are markedly higher than in Group Ro I, the group which never subsequently learned the criterion discriminations. However, the results of the LS/Ro group suggest that activity is not the only nor the most important factor involved.

It will be recalled that these subjects had attained a significant degree of learning-set formation under the control condition prior to their transfer to the experimental condition. However, neither their previous discrimination experience, nor their relatively high levels of pedal pressing had any facilitatory effect upon the subsequent acquisition of observing responses. Indeed, they not only showed a decline in discrimination learning, but failed to develop any consistent relationship between pedal pressing and discrimination performance (Fig. 9). The persistence of this breakdown in performance over the large number of problems presented makes it unlikely that the breakdown is attributable solely to the extreme novelty of the observing response condition for the control subjects.

Furthermore, the results of a preliminary study, not reported in this paper, provide some confirmation for the results of the LS/Ro group. This study showed that even in subjects with considerable previous training under observing response conditions, the acquisition of a high degree of discrimination on a given problem under control conditions did not markedly facilitate performance on this same problem under observing response conditions. Taken together, these results suggest that a high degree of discrimination, acquired independently of the operant observing response, may not be sufficient to produce those orderly changes in observing response development upon which further increases in discrimination learning are dependent.

Certain other aspects of the present findings also appear to contradict the general hypothesis underlying the concept of observing responses. This hypothesis states that "exposure to discriminative stimuli will have a reinforcing effect upon the observing response to the extent that the subject has learned to respond differently to the discriminative stimuli" (Wyckoff, 1952). In considering the reinforcement mechanism through which the degree of discrimination learning effects secondary response development, Wyckoff suggested that this mechanism involved reinforcement and/or changes in the probability of reinforcement. However, there is evidence that increases in observing responses may occur under conditions where neither of these mechanisms is likely to be operative, e.g. in situations where no improvement in discrimination learning is apparent. Thus in Figure 2 and Figure 8 increases in observing responses are occurring prior to any noticeable improvement in discrimination learning.

This apparent contradiction may be resolved if we assume that a highly preferred stimulus may provide its own reinforcement for the observing response, independently

of subsequent food reinforcement. Support for such an assumption comes from a number of subsidiary observations. These indicate that a marked preference for either of the stimuli in a given problem which produces differential responding to the stimuli, also tends to produce a high level of observing responses during the problem. The correctness or incorrectness of the preferred stimuli seems to be irrelevant. Such observations suggest that both *learned* and *unlearned* differential responses to the discriminative stimuli will have a reinforcing effect upon the observing response.

In conclusion, it is worth noting that the concept of observing responses has much in common with Lawrence's concept of "mediating responses," as it is used in his experiments on the "acquired distinctiveness of cues," (Lawrence, 1949, 1950). In the light of this relationship, one other similarity between the present data and Lawrence's position may be noted. Lawrence (1958) suggests that the readiness with which a mediating response is associated with a given physical stimulus "is to a large extent determined by the nature of that stimulus, the context in which it occurs and the innate makeup of the organism . . . but the degree of distinctiveness obtained can be varied by the type of training involved." Such a formulation is consistent with the present finding that both learned and unlearned differential behaviour may reinforce the observing response.

The concept of "observing responses," therefore, may have theoretical implications, for the problem of curiosity, exploratory behaviour, selective attention, etc., while the use of experimental analogues for "observing" may be of value in future research on such problems.

REFERENCES

- GRANT, D. A. (1956). Analysis-of-variance in the analysis and comparison of curves. *Psychol. Bull.*, **53**, 141-54.
- HOLLAND, J. G. (1957). Technique for the behavioral analysis of human observing. *Science*, **125**, 348-50.
- KELLEHER, R. T. (1958). Stimulus-producing responses and attention in the chimpanzee. *J. exp. anal. Behav.*, **1**, 87-102.
- LAWRENCE, D. H. (1949, 1950). Acquired distinctiveness of cues, I and II. *J. exp. Psychol.*, **39**, 770-84; **40**, 175-88.
- MORRELL, F., and JASPER, H. H. (1956). Electrographic studies of the formation of temporary connections in the brain. *Electroenceph. clin. Neurophysiol.*, **8**, 201-16.
- PUBOLS, B. H. Jr. (1956). The facilitation of visual and spatial discrimination reversal by overlearning. *J. comp. physiol. Psychol.*, **49**, 243-8.
- REID, L. S. (1953). The development of non-continuity type behaviour through continuity learning. *J. exp. Psychol.*, **46**, 107-12.
- SOKOLOV, E. N. (1956). Higher nervous activity and the problem of perception. In B. Simon (Ed.), *Psychology in the Soviet Union*. Stanford. Pp. 92-9.
- WYCKOFF, L. B. Jr. (1951). The role of observing responses in discrimination learning. Unpublished Ph.D. dissertation. Indiana University.
- WYCKOFF, L. B. Jr. (1952). The role of observing responses in discrimination learning. *Psychol. Rev.*, **59**, 431-41.

STUDIES ON SENSORY FEEDBACK

I: Effect of Delayed Auditory Feedback on Speech and Keytapping

BY

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The effects of delayed auditory feedback (DAF) on the performance of simple speech and keytapping tasks were compared. In the first experiment, each of the fourteen young and adult subjects was asked to repeat the speech sound "b" in groups of three sounds. The subject heard his undelayed speech sounds through earphones. Under the delay condition, the speech sounds were delayed 244 millisecc. before being returned to the subject's earphones. In the second experiment, each of the same subjects was asked to tap on a key in groups of three taps. The subjects heard an undelayed click through earphones each time they tapped on the key. Under the delay condition, the clicks were delayed 244 millisecc. before being returned to the earphones. Graphic recordings were made of the time and intensity characteristics of tapping under both conditions.

The changes which occurred in speech and keytapping under DAF were qualitatively the same and consisted of increases in intensity and unit-to-unit time of both taps and speech sounds. When performing under DAF, the subjects also exhibited a tendency to repeat more sounds or to tap more times than had been requested. The percentage of change in intensity and time characteristics for speech and keytapping performances which occurred under DAF were not significantly different for the two motor systems. The amount of change in speech performance under DAF did not correlate significantly with the amount of change in keytapping performance.

The idea that sensory information coming from muscles to the central nervous system plays an important rôle in the control of movement was first clearly stated by Sir Charles Bell in 1826 (Bell, 1844). Since that time, it has become clear that sensory information is particularly important for normal *voluntary* movement. As the experiments of Sherrington and Mott on monkeys have shown, non-voluntary types of movement can still occur in a deafferented limb (Sherrington and Mott, 1895); and it has recently been demonstrated that monkeys can relearn a conditioned reflex requiring flexion of a limb which has been completely deafferented (Knapp, Taub and Berman, 1958).

Further evidence of the importance of sensory information for the control of voluntary movement is afforded by clinical observations of the speech difficulties of deaf patients and of the ataxia which follows lesions in dorsal roots and dorsal column (as in *tabes dorsalis*).

The fact that sensory information is vital for initiation and control of normal voluntary movement suggests that the sensory information which is produced as a function of movement is used by the nervous system during the course of movement. The concept of sensory information from a motor performance functioning as a persistent controlling influence on the motor performance fits the formal model of a servo-system developed by engineering theory (Shannon and Weaver, 1949; Wiener, 1948). In this model, which has been fully described by a number of authors, the

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sensory information from a motor performance is called "feedback" (Fitts, 1951; Gibbs, 1954; Gooddy, 1949; Hammond, Merton and Sutton, 1956; Ruch, 1951).

This theoretical model, in which sensory feedback is considered to function as a controller or monitor of movement by supplying information to central neural structures, has been applied to the experimental analysis of movement. A large series of studies has been done on the rôle of proprioceptive sensory feedback in the regulation of voluntary movements such as a simple tracking movement (Bahrick, 1957; Bahrick, Fitts and Schneider, 1955; Gibbs, 1954; Hefferline, 1958).

The idea that sensory feedback is used to monitor movement has not been universally accepted (Lashley, 1951). However, the arguments against sensory feedback theory seem to be concerned not with the general usefulness of the model, but with the question of the mechanism by which feedback influences motor control (Gibbs, 1954; Lashley, 1951).

In the present argument, we shall assume that sensory feedback is used by central neural structures for the control of voluntary movements. We are considering sensory input to the organism as a form of "information" about a motor event which is somehow used to control that event. Alteration in this sensory feedback information would accordingly be expected to result in a change in the motor performance in question.

A number of studies have been done on the effects of delayed auditory feedback (DAF) on speech (Chase, Sutton and First, 1959). All of them have shown that speech is markedly changed when auditory feedback is delayed. The most striking changes in speech under DAF are increases in vocal intensity, increases in phonation time, and a tendency to repeat speech sounds (Fairbanks, 1955; Chase, 1958).

In the present study, we shall consider the effect of delayed auditory feedback (DAF) on the time and intensity of speech and keytapping. Our experiments were designed to assist in answering several questions: (a) Are the alterations in time and intensity of speech and of keytapping under DAF qualitatively the same for the two motor systems? (b) Does the same delay in auditory feedback result in more change in motor performance for one system than for the other? (c) Does the amount of change in a given subject's speech under DAF correlate with the amount of change in his keytapping under DAF?

METHOD

Subjects

The subjects were fourteen young adults, students and personnel at the Columbia-Presbyterian Medical Centre, New York City.

Procedure and calibration

Speech. The subject was seated at a desk in a sound-treated room and fitted with PDR-1 earphones with U.S. Air Force Type MX-41/AR rubber ear cushions. He was placed so that his mouth was directly opposite an Altec Lansing 21C microphone. A head rest was adjusted so that the subject's lips remained 4 in. from the microphone. The subject was instructed to repeat the speech sound "b" (as in book), in groups of three, until he had uttered seven to ten such groups.

Under the first set of conditions, the output from the microphone fed into an amplification system (a Presto 900-A4 and a Bogen DB-110 amplifier) which added 72 dB. to the subject's speech signal before it was returned to his earphones.

Under the second set of conditions, the instruction and procedure just described were again used, but this time the signal from the microphone passed through the Presto amplifier to the record head of a Presto RC-11 tape recorder. The speech signal was recorded on magnetic tape, and passed at a speed of 15 in. per sec. to the playback head, from which it passed to the Bogen amplifier, and then into the subject's earphones.

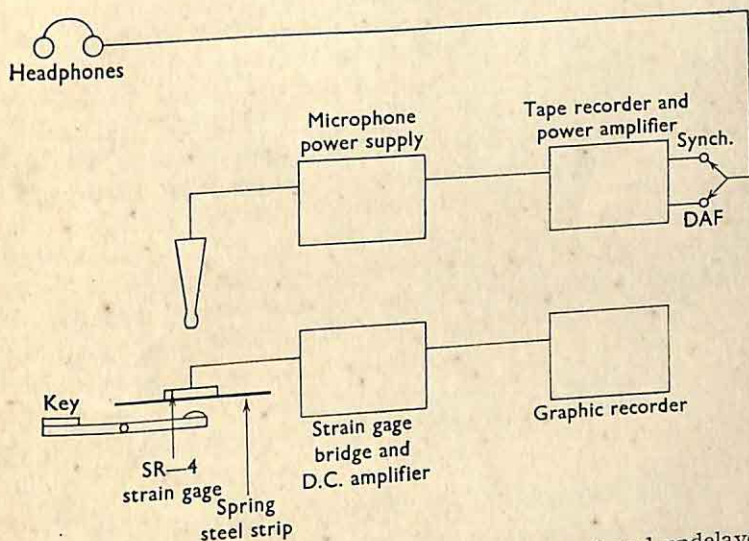
A movable pin placed between the record and playback heads permitted the operator to create a loop of tape, and thereby to vary the distance the tape travelled from record to

playback head. This distance was adjusted to give a feedback delay time of 244 millisecc. at the tape speed used. As under the first condition, a gain of 72 dB. was added to the subject's speech signal before it was returned to his earphones.

The amplification of auditory feedback under delayed return was duplicated under synchronous (or undelayed) return by using a 0.9 mV., 1,000 c.p.s. signal from a Heath AG-9A signal generator as the calibrating input to the system. Amplification was adjusted so that output voltage from the system would reach the desired control value (0.35 V.). The Allison 3A Audiometer Calibration Unit was used to determine the acoustical output of the system composed of the Altec microphone, Presto amplifiers, and PDR-1 receivers. (The total acoustic gain of the system, 50 dB, was computed by subtracting the average acoustic pressure level for normal speech (ca 70 dB. re: 0.0002 μ bar) from the acoustic output of the Allison Audiometer Calibration Unit. It should be noted that the system gain is less than the amplifier gain because of a degradation of the signal caused by inefficiency in the transducers used.)

Keytapping. The subject, again seated at a desk in a sound-treated room, and fitted with the same earphones, was asked to tap on a key in groups of three taps until he had tapped seven to ten groups. The key (Fig. 1) consisted of a brass bar through which a

FIGURE 1



Schematic diagram of equipment used to provide both delayed and undelayed auditory feedback.

supported fulcrum passed. When the subject hit one end of the bar, the other end moved up and struck a strip of spring steel on which was mounted an SR-4 strain gage. Each time the subject tapped on the brass bar, and thereby caused it to strike the steel strip, a sound was produced. The sound was clearly audible in the free field, and increased in intensity with the increasing strength of the tap. The sounds produced by each tap on the key were picked up by an Altec Lansing 21C microphone which was placed near the point of contact between the bar and the steel strip. (Unfortunately, the mechanics of the instrument were such that a soft click was produced just before the brass bar hit the steel strip and again just as it began to return to resting position. These clicks were of constant intensity regardless of the strength with which the key was tapped.)

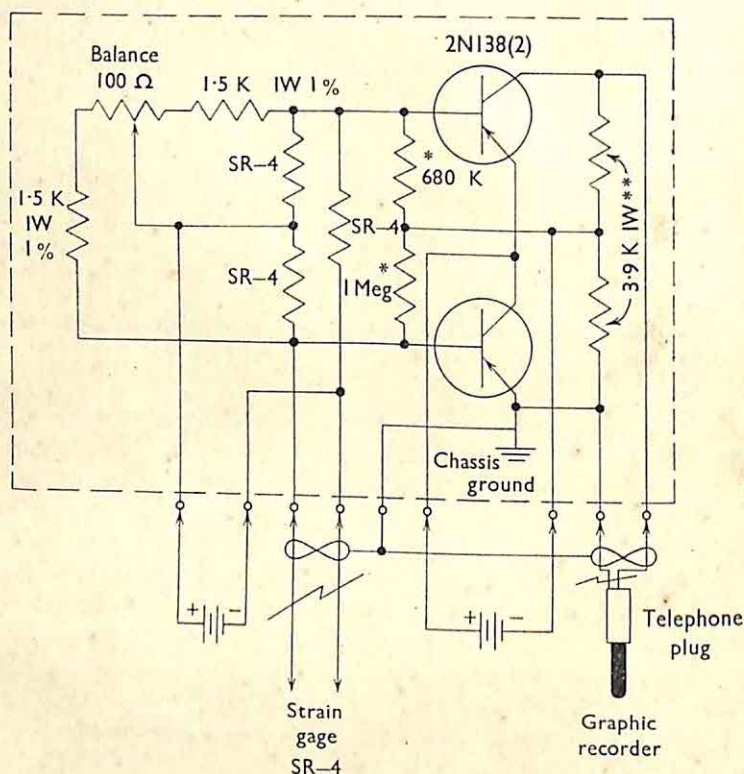
Under the first set of conditions of the keytapping experiment, the output from the microphone fed into an amplification system (as above) which added 72 dB. to the sound produced by tapping on the key before it was returned to the subject's earphones.

Under the second set of conditions, the same instructions and procedure were used, but this time the sounds produced by the keytapping were delayed 244 millisecc. before being returned to the subject's earphones. The feedback was delayed in the same manner

as described above, and the sounds again were amplified equally under delayed and under synchronous feedback.

Under both sets of conditions, a continuous recording of the time and of the amplitude of taps were made. The output of the strain gauge mounted on the steel strip was amplified, and the amplified signal used as the input to a Sanborn "Viso Cardiette" No. 562M direct writing galvanometer (Figs. 1 and 2). The displacement of the galvanometer

FIGURE 2



Circuit diagram of D.C. amplifier and strain gauge bridge.

pen was directly proportional to the force exerted on the steel strip. The linearity of the pen response to pressure exerted on the steel strip was determined by placing weights of known value (ranging from 50 to 1,200 gm.) on the brass bar. Pen displacement was then plotted for each weight, the sensitivity of the galvanometer having been set before each experiment so that a 500-gm. weight resting on the brass bar produced a 15-mm. displacement of the pen. As a check on the stability of the system's linearity, 200-, 500-, and 1,000-gm. weights were placed on the brass bar before and after each subject was run. The pen displacement was measured for each weight and checked for correspondence to the linear calibration curve. A maximum deviation of 2.0 mm. for 500 and 1,000 gm. and 1.0 mm. for 200 gm. was accepted. To make sure that the paper-driving motor maintained a constant speed, five pulses from a 1 per sec. timing mechanism were placed at the beginning and the end of each subject's record, and measured.

Analysis

Speech. The magnetic tape recordings of each subject's speech were used as the input to a Tektronix Type 532 cathode-ray oscilloscope. The resulting display was photographed with a Grass C4 oscilloscope recording camera (film speed = 25 mm. per sec.). The distance between the beginning of each speech sound and the beginning of the next, within each group of three sounds, was defined as the "unit-to-unit distance." This

distance was measured for the third through the seventh groups of sounds in each condition of the experiment. In some cases, the subject did not leave pauses adequate to permit clear delineation of groups of sounds. In such cases, the distance from sound to sound was measured starting with the seventh sound, to give fifteen consecutive measurements. The film speed value was used to convert "unit-to-unit distance" to "unit-to-unit time." It is the latter measure which is used in the statistical analysis.

The outline of each speech sound display was drawn with a pencil directly on the oscillograms. The resulting "envelope" defined the contour of the display of each sound. The distance across each envelope at its widest point (measured perpendicular to the time axis) was defined as the "intensity" of the speech sound. Starting with the seventh sound display on the oscillogram, the intensity of fifteen consecutive speech sounds was measured. The measurements were made in millimeters, but since the height of the sound display on the oscillogram is directly proportional to sound intensity, we express the measurement as arbitrary units of sound intensity. The control dials on both tape recorder and oscilloscope were fixed for the filming of both conditions of the speech experiment, thus permitting the intensity of sounds (in arbitrary units) to be interpreted relative to each other but not with reference to an absolute unit of sound intensity.

Keytapping. The distance between the first downward deflection of the galvanometer pen and the next, within each group of three taps, was defined as "unit-to-unit distance." This distance was measured for the third through the seventh groups of taps under each condition of the experiment. For cases in which groups of taps could not be distinguished, the distance from tap to tap was measured to give fifteen consecutive measurements, starting with the seventh tap. The chart speed value was used to convert "unit-to-unit distance" into "unit-to-unit time." The latter measure was used in the statistical analysis. The greatest distance which the galvanometer pen moved from the baseline during each tap was defined as the "intensity" of the tap. Since the distance of the pen displacement is directly proportional to force exerted on the key, the millimeters of pen displacement can be considered as arbitrary units defining the intensity of each tap.

RESULTS

The changes in motor behaviour which occurred most often under DAF were the same for both speech and keytapping tasks. The changes consisted of increased intensity, increased unit-to-unit time, and a tendency to make errors in number (Tables I, II and III). An error in number, for example, would be to tap four times instead of three, or to repeat a sound four times instead of three. The adding of one extra unit was the most common type of error in number for both speech and keytapping under DAF (Table III).

TABLE I
COMPARISON OF GROUP DATA ON PERFORMANCE OF SPEECH AND KEYTAPPING
TASKS UNDER SYNCHRONOUS AND DELAYED AUDITORY FEEDBACK (N = 14)

	Key		Speech	
	Synchronous	DAF	Synchronous	DAF
Intensity (arbitrary units)				
Mean*	10.5	12.1	6.0	7.2
	$p < 0.01$		$p < 0.01$	
Range	3.5-16.7	5.5-18.8	2.7-9.6	3.5-10.3
Unit-to-unit time (sec.)				
Mean*	0.37	0.50	0.35	0.56
	$p < 0.001$		$p < 0.05$	
Range	0.17-0.48	0.16-0.63	0.14-0.73	0.17-1.97

Note.—*t* tests were employed to test significance.

* This is the group mean for this measure.

TABLE II

MEAN PERCENTAGE OF CHANGE IN INTENSITY AND UNIT-TO-UNIT TIME
UNDER DELAYED AUDITORY FEEDBACK (N = 14)

	<i>Key</i>	<i>Speech</i>	<i>p</i> *
Intensity (arbitrary units) ..	20.5	27.1	NS
Unit-to-unit time	35.4	47.7	NS

**t* tests were used.

TABLE III

ERRORS IN NUMBER IN THE PERFORMANCE OF SPEECH AND KEYTAPPING
TASKS UNDER SYNCHRONOUS AND DAF

	<i>Key</i>		<i>Speech</i>	
	<i>Synchronous</i>	<i>DAF</i>	<i>Synchronous</i>	<i>DAF</i>
Total errors	0	4	0	22
Number of subjects who made errors	0 (N = 12)	2 (N = 12)	0 (N = 14)	8 (N = 11)

The changes in intensity under DAF for keytapping and speech are shown in Table I. The mean intensity of the taps under synchronous feedback was 10.5, this value being the mean of the individual subjects' mean scores for the intensity measure. Under DAF the mean intensity rose to 12.1. The difference between these two values was significant at the 0.01 level. The percentage of change in mean intensity from synchronous to DAF was determined for each subject by subtracting his mean intensity score under synchronous from his mean intensity score under DAF and dividing by the mean intensity score under synchronous. The percentage of change scores were averaged for the total population (Table II). The percentage of change in the mean intensity of keytapping caused by DAF was 20.5.

The mean intensity of speech sounds increased from 6.0 under synchronous to 7.2 under DAF. The difference between these two values was significant at the 0.01 level. The percentage of change in mean intensity of speech from synchronous to DAF was 27.1 (Table II).

Changes in unit-to-unit time (in seconds) for keytapping are also shown in Table I. The mean unit-to-unit time for keytaps was 0.37 under synchronous feedback, and rose to 0.50 under DAF. These values are the means of the individual subjects' mean scores for the unit-to-unit time measure. The difference between these two values was significant at the 0.001 level. The percentage of change in unit-to-unit time for keytapping as a result of delay was 35.4. The mean unit-to-unit time for speech sounds increased from 0.35 under synchronous to 0.56 under DAF. The difference between these two values was significant at the 0.05 level. The percentage of change in unit-to-unit time for speech under DAF was 47.7. It will be noted that the upper limits of the range for unit-to-unit time shifted upward under DAF for both key and speech tasks, and this, combined with the relative fixity of the lower limits, resulted in a marked increase in the absolute range under DAF.

The unit-to-unit time is a composite measure. It consists of the time from the beginning to the end of a unit as well as the time from the end of one unit to the beginning of the next, for both speech and keytapping. The increase in unit-to-unit

time under DAF reflects the tendency of both the time of each unit, and the time from the end of one unit to the beginning of the next, to increase when speech or keytapping is performed under DAF. Technical difficulties prevented quantitative analysis of the unit-to-unit time in terms of its components.

There were no significant differences between the percentage of change in keytapping and the percentage of change in speech for either measure (Table II).

Errors in number occurred for both keytapping and speech under DAF (Table III). Each set of conditions of the keytapping experiment called for five groups of three taps each. Any group which contained more or less than three taps was scored as one error in number. The same procedure was applied to the scoring of errors in number for the speech experiment. The total errors in number, and the number of subjects who made errors, are shown for each set of conditions. The N often dropped below the total number of subjects tested, because several subjects did not leave adequate pauses between groups of taps or sounds; an error in number can only be detected if the groups are clearly distinguished. The N for each condition, therefore indicates the number of subjects who did distinguish groups clearly for that condition. It will be noted that errors in number occurred more frequently in speech under DAF than in keytapping under DAF.

The scores for mean intensity under synchronous feedback correlated significantly with the mean intensity scores under DAF for both speech and keytapping (Table IV). The mean unit-to-unit time scores under synchronous feedback correlated significantly with the unit-to-unit time scores under DAF for both speech and keytapping.

TABLE IV
PEARSON PRODUCT-MOMENT CORRELATION COEFFICIENTS

	<i>Key (DAF)</i> <i>Key (synch.)</i>	<i>Speech (DAF)</i> <i>Speech (synch.)</i>	<i>Speech (Synch.)</i> <i>Key (synch.)</i>	<i>Speech (DAF)</i> <i>Key (DAF)</i>	<i>Speech (percentage change). Key (percentage change)</i>
Intensity.	0.92**	0.80**	0.30	0.14	0.28
Unit-to-unit time (sec.)	0.79**	0.84**	0.46	0.30	0.14

** $p < 0.001$.

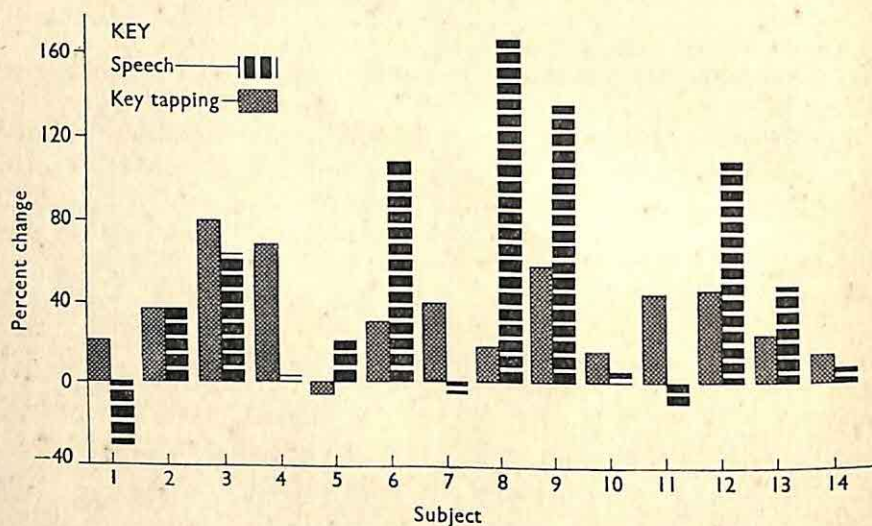
$r \geq 0.53$ ($p < 0.05$).

There were no significant correlations between speech and keytapping scores under either condition of the experiment; nor did the percentage of change in speech under DAF correlate significantly with the percentage of change in keytapping under DAF either for mean intensity or for mean unit-to-unit time scores. In Figure 3, the percentage of change under DAF in mean unit-to-unit time for speech and keytapping is plotted by subject; in Figure 4, the percentage of change in mean intensity under DAF is plotted in the same way; and in both cases, the lack of correlation between scores for speech and keytapping is evident.

DISCUSSION

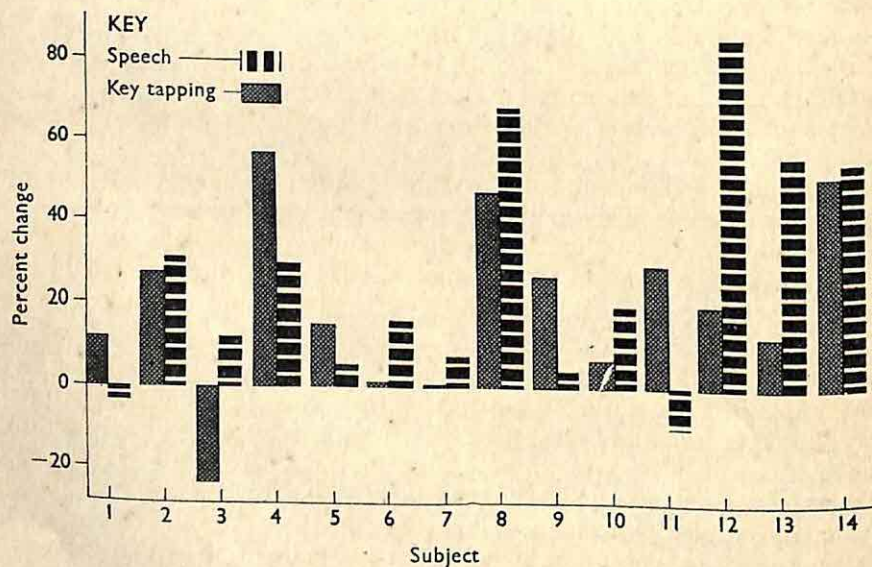
Both keytapping and speech performances were significantly changed when auditory feedback was delayed. The changes in performance were qualitatively the same for both motor systems: they consisted of increases in intensity and in unit-to-unit time, and a tendency to make errors in the number of responses. The qualitative

FIGURE 3



Percentage of change in mean unit-to-unit time for speech and keytapping tasks plotted by subject.

FIGURE 4



Percentage of change in mean intensity for speech and keytapping tasks plotted by subject.

similarity suggests that the same temporal alteration in auditory feedback might have similar effects on many widely different motor systems; it remains for further experiments to determine whether this is so, and whether delayed feedback for other sensory modalities operates on behaviour in the same way as delayed auditory feedback.

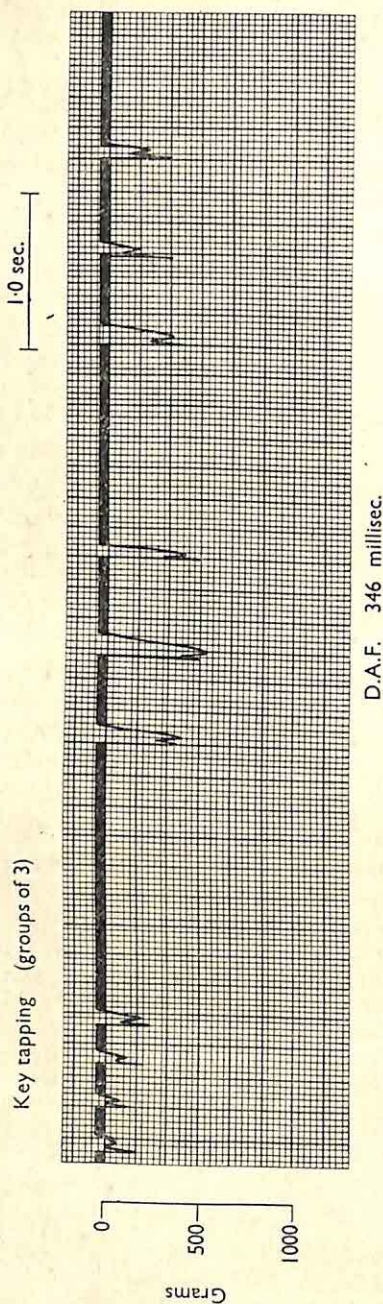
Our results suggest that there are critical time periods within which the sensory feedback accompaniments of a motor operation must be received if optimal motor control is to be maintained. It would seem reasonable to suppose that the changes in motor performance which result from a delay in auditory feedback are not only a direct expression of alteration in the sensory information used to monitor the performance, but also represent compensatory efforts to restore control of the disturbed motor performance. The qualitative similarities in performance of the two motor functions under DAF may indicate that similar compensatory mechanisms operate to restore control in both systems.

It is possible that the increased intensity and the increased unit-to-unit time which are characteristic of speech and keytapping under DAF operate to increase the amount of sensory feedback returning along other, undisturbed channels, e.g. tactile and proprioceptive. We frequently observed that marked increases in intensity and unit-to-unit time of tapping occurred just after an error in number of responses was made. In Figure 5, for example, we have the keytapping record of a subject who has been requested to make groups of three taps each under DAF. The first part of the record shows the performance of the subject at the time he was first exposed to a delay in auditory feedback. The intensity and unit-to-unit time of his taps were close to those obtained under control conditions; but he made an error in number of responses by tapping four times instead of three. In the next group of taps, we observed a striking increase in intensity and unit-to-unit time, but the subject now tapped the correct number of times. In the third group of taps, the subject tapped at intensities which were intermediate between the control values and the large values for the group which immediately followed the error in number. The unit-to-unit time values remain unchanged. The return to lower intensities in the third group may indicate that the total amount of unimpaired sensory feedback information needed to maintain optimal control of the motor performance under DAF was found by a method of overshoot.

The percentage of change in intensity under DAF was nearly the same for both keytapping and speech; the same was true of unit-to-unit time. We know that normal control of speech is quite dependent on auditory feedback, for deaf patients have great difficulty in monitoring their speech. However, the performance of some kinds of manual tasks produces little auditory feedback, and tapping performances can be carried out with considerable skill even when the taps are too light to produce much sound. It would seem from these facts that speech could be expected to be more dependent on an unaltered auditory feedback than keytapping; we have seen, however, that DAF produced changes in the two systems which are quantitatively quite similar. (It must be kept in mind that the tasks performed in the speech and keytapping motor systems were both very simple, and extrapolation from the present results to more complex performances is not justified.)

Perhaps a delay in sensory feedback results in marked alteration in motor performance whether or not this motor control system normally depends on the specific type of sensory information which is delayed. It is likely that the disturbance in performance which occurs with a delay in auditory feedback is greater than would result from a complete removal of auditory feedback. Each motor system has many sensory feedback channels, and many motor control systems can probably withstand

FIGURE 5



A graphic recording of the time and intensity characteristics of keytapping under delayed auditory feedback.

a vastly diminished flow of information along one or more of the sensory feedback channels. On this point, we may consider the experiment of Lassek in which only one dorsal root of a monkey's brachial plexus was left intact (Lassek, 1953). Despite the extensive sensory loss, the monkey suffered only a slight impairment of motion, whereas section of all the dorsal roots of the brachial plexus results in total flaccid paralysis immediately after operation.

The striking difference in the amount of errors in number which occur in speech and keytapping under DAF (Table III) suggests that audition may be more critical for the mechanisms which effect the sequential release of motor units in speech than for the mechanisms which effect release of motor units for tapping. Tactile or proprioceptive feedback may be more important for the control of sequential release of taps.

The low correlation between the amount of change a subject shows in keytapping under DAF and the amount of change he shows in speech under DAF indicates that the disturbance potential of a delay in auditory feedback for a given subject varies with the specific motor task in question. It is possible that each person learns a given motor skill by integrating sensory feedback information in a highly individual manner. The types of sensory feedback which are integrated in the process of learning motor skills may determine the specific pattern of sensory feedback necessary to monitor the motor task. Characteristic learning patterns for motor skills would, therefore, result in characteristic patterns of sensory feedback monitoring.

Our discussion has thus far considered the immediate findings of the present experiments. We should like to mention two possible applications of this type of research.

Precise understanding of the changes which occur in motor performance as a result of specific sensory feedback alterations may give us a diagnostic tool for the appraisal of disturbed functioning of the neural structures which make up the different sensory feedback monitoring loops. Such understanding, particularly of possible compensatory mechanisms reflected in these changes, is also pertinent to the question of prostheses for sensory losses. If a motor deficit results from a sensory lesion, such as the ataxia of dorsal column disease, we can perhaps increase motor control by supplying the organism with types of sensory feedback other than those which can no longer pass to the central neural structures.

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REFERENCES

- BAHRICK, H. P. (1957). An analysis of stimulus variables influencing the proprioceptive control of movements. *Psychol. Rev.*, **64**, 324-8.
- BAHRICK, H. P., FITTS, P. M. and SCHNEIDER, R. (1955). Reproduction of simple movements as a function of factors influencing proprioceptive feedback. *J. exp. Psychol.*, **49**, 445-54.
- BELL, C. (1844). On the nervous circle which connects the voluntary muscles with the brain. *Philos. Trans.*, 1826, pt. 2, 163-175. In Bell, C., *The Nervous System of the Human Body*. London. Pp. 193-203.
- CHASE, R. A. (1958). Effect of delayed auditory feedback on the repetition of speech sounds. *J. Speech Dis.*, **23**, 583-90.

- CHASE, R. A., SUTTON, S., and FIRST, D. (1959). Bibliography: delayed auditory feedback. *J. Speech Hearing Res.*, **2**, 193-200.
- FAIRBANKS, G. (1955). Selective vocal effects of delayed auditory feedback. *J. Speech Dis.*, **20**, 333-45.
- FITTS, P. M. (1951). Engineering psychology and equipment design. In S. S. Stevens (Ed.), *Handbook of Experimental Psychology*. New York. Pp. 1287-340.
- GIBBS, C. B. (1954). The continuous regulation of skilled response by kinaesthetic feedback. *Brit. J. Psychol.*, **45**, 24-39.
- GOODDY, W. (1949). Sensation and volition. *Brain*, **72**, 312-39.
- HAMMOND, P. H., MERTON, P. A., and SUTTON, G. G. (1956). Nervous gradation of muscular contraction. *Brit. med. Bull.*, **12**, 214-8.
- HEFFERLINE, R. F. (1958). The role of proprioception in the control of behaviour. *Trans. N.Y. Acad. of Sciences*, **20**, Ser. II, 739-64.
- KNAPP, H. D., TAUB, E., and BERMAN, A. J. (1958). Effect of deafferentation on a conditioned avoidance response. *Science*, **128**, 842-3.
- LASHLEY, K. S. (1951). The problem of serial order in behaviour. In L. A. Jeffries (Ed.), *Cerebral Mechanisms in Behaviour*. New York.
- LASSEK, A. M. (1953). Potency of isolated brachial dorsal roots in controlling muscular physiology. *Neurology*, **3**, 53-7.
- RUCH, T. C. (1951). Motor systems. In S. S. Stevens (Ed.), *Handbook of Experimental Psychology*. New York. Pp. 154-208.
- SHANNON, C. E., and WEAVER, W. (1949). *The Mathematical Theory of Communication*. Urbana.
- SHERINGTON, C. S., and MOTT, F. W. (1895). Experiments upon the influence of sensory nerves upon movement and nutrition of the limbs. *Proc. roy. Soc.*, **57**, 481-8.
- WIENER, N. (1948). *Cybernetics: or Control and Communication in the Animal and the Machine*. New York.

STUDIES ON SENSORY FEEDBACK

II Sensory Feedback Influences on Keypapping Motor Tasks

BY

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Subjects were trained to tap a key continuously at a specific rate, and with a specific amount of pressure (regularity task). Performance of this task was studied under conditions of: (a) decreased auditory feedback (masking noise through earphones), (b) decreased visual feedback (tapping hand screened from view), (c) vibration (vibrators applied to forearm in order to "mask" proprioceptive feedback), (d) digital block of tapping finger, and (e) combination of all four conditions. Significant changes in rate and intensity of tapping resulted under conditions of decreased auditory feedback, vibration, and the combined condition.

In the second part of the study, the effects of different delayed sensory events on keypapping were examined. The five conditions of delayed sensory feedback were: (a) delayed auditory feedback, (b) delayed visual feedback, (c) delayed tactile feedback, (d) the first three delayed sensory events presented simultaneously, and (e) condition (d) repeated with digital block of the tapping finger.

The conditions of delayed sensory feedback did not markedly alter performance of the regularity task. The same conditions of delayed sensory feedback did, however, produce highly significant changes in the performance of a more complex pattern task. All of these delay conditions produced parallel changes in the pattern task, namely increased intensity and decreased rate of tapping. The fact that the pattern task is more disturbed by delayed sensory feedback than the regularity task suggests that temporal complexity of the task is one determinant of the degree to which it will be disturbed by a delay in sensory feedback.

During the last 60 years, both clinical and experimental evidence have indicated that sensory information is critical for the learning, initiation, and continuous control of voluntary motor activity. Reviews of the pertinent theoretical and experimental considerations are readily available (Adrian, 1947; Bastian, 1909; Denny-Brown, 1950; Fitts, 1951; Lassek, 1953; Sherrington, 1952; Twitchell, 1954; Walshe, 1947).

The idea that the sensory information associated with movement is utilized to control the motor responses fits the formal model of a servosystem developed in engineering theory (Fitts, 1951; Melton and Briggs, 1960; Ruch, 1951). In this model, the sensory information produced by an ongoing motor activity is called "feedback." This sensory information returns to the central nervous system and is used to control the ongoing motor activity.

In earlier studies, we have investigated the effects of a delay in air-conducted auditory feedback on speech (Chase, 1958; Chase, Sutton and First, 1961). We later began to study simple keypapping motor performances because this type of motor performance permitted experimental manipulation of more sensory feedback channels and was amenable to more precise quantification of motor changes. In one study, we compared the effect of delayed air-conducted auditory feedback on analogous tasks in speech and keypapping (Chase, Harvey, Standfast, Rapin and Sutton, 1959). We found the changes in performance caused by the delayed auditory feedback to be the same for these two different motor systems. When performing under delayed auditory feedback, the subject increases the intensity of each response (the speech sound is louder; the key is hit harder), the duration of the response is increased, the rate of response decreases, and the subject frequently repeats responses.

In the present study, we have undertaken to determine whether delay in sensory channels other than the auditory produce similar changes in keytapping performance; studies were carried out, therefore, with delayed auditory, visual, and tactile feedback. We have also examined the effects of reduction of visual, auditory, tactile, and proprioceptive feedback on keytapping.

METHOD

Instrumentation and procedure

In this study, the effects of different sensory feedback alterations on the rate and amplitude characteristics of practiced keytapping performances were studied. The subject was seated next to a table, with his arm resting on a wooden block, and his index finger placed just above a key on which he tapped.

Subjects. The subjects were fifteen normal young adults who were almost all medical students or personnel at the Columbia-Presbyterian Medical Centre in New York City.

Key. The key on which the subjects tapped consisted of a hard-rubber dowel glued to a spring-steel hacksaw blade. The blade was clamped on supports at both ends, and the dowel was supported above the blade by a metal bridge. A metal electrode was glued to the top of the dowel, and the subject held a grounded electrode in his free hand. This arrangement permitted the subject to trigger a switch each time he tapped on the key. The apparatus for recording tapping performance was identical with that described in our previous paper (Chase, Harvey, Standfast, Rapin and Sutton, 1961).

Keytapping tasks

Two keytapping tasks were used in this study.

Regularity task. The subject was asked to tap as regularly as possible with respect to rate and amplitude, and was instructed to follow the rate of an electric metronome, which beat 3 times per sec. Cues for learning the amplitude to be maintained were provided by connecting a voltmeter across the input to the direct-writing galvanometer. The deflection of the meter needle was directly proportional to the force of each tap. Subjects were instructed to deflect the needle within a range which was equivalent to a static force on the key of 60-gm. at the low end, and equivalent to 90-gm. of static force on the key at the high end.

Pattern task. The pattern task consisted of tapping on the key in groups of three taps each. Subjects were again requested to maintain maximum constancy of rate and amplitude of tapping. The metronome beat at a rate of 3 per sec., and the subject was instructed to tap in rhythm with each of three beats, pause for the next two, tap in rhythm with the next three, and so on in this fashion. The subjects were trained to tap at the same amplitude as in the regularity task.

Experimental conditions of decreased sensory feedback

Decreased auditory feedback. PDR-1 earphones mounted in Wilson No. 258 "sound barrier" liquid seal enclosures were placed over the subject's ears, and sound from a Piling Witting sawtooth noise generator was fed into the phones at sufficient gain to completely mask the mechanical sounds associated with tapping.

Decreased visual feedback. A screen was placed so that the subject could not see his hand while tapping on the key.

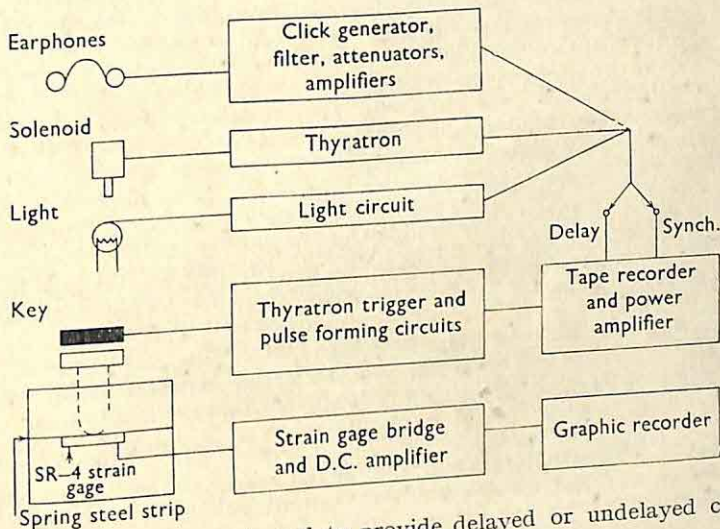
Vibration. We desired to approximate a condition of diminished proprioceptive feedback. Vibratory sensory information enters the dorsal columns of the spinal cord along with the proprioceptive information. We felt that vibration on the arm would distort the decoding of the proprioceptive information by the central nervous system because of its randomness with respect to the tapping, and because of the preponderance of vibratory over proprioceptive activity which would result from using enough vibrators. Vibrators were held on the subject's wrist and forearm. The subject held a roll of tape in his hand in order to keep the wrist in a position of dorsiflexion and thus prevent the increase in momentum of the tapping finger due to the vibration.

Digital block. One c.c. of 1 per cent. xylocaine was injected on each side of the index finger near the base of the proximal phalanx. This procedure abolished light touch and pin prick sensation in 5 to 15 min.

Experimental conditions of delayed sensory feedback

Each time the subject tapped on the key he closed a switch, and an electrical pulse was discharged into a Presto RC-11 magnetic tape recorder. The signal was recorded on tape, and passed at a speed of 15 i.p.s. to the playback head. A moveable pin, placed between the record and playback heads, permitted the experimenter to create a loop of tape, and thereby to increase the distance the tape travelled from record to playback head. The distance was fixed to give a delay time of 300 millisecc. from record to playback head. The electrical pulse registered at the playback head, and then triggered the circuit used to produce light flashes, clicks, and tactile pulses, either singly or in combination (Fig. 1). This procedure permitted the experimenter to provide the subject with visual, auditory and tactile sensory events 300 millisecc. after each tap on the key. These three types of sensory events could be provided without delay (or synchronously with each tap) by passing the electrical pulse triggered by the tap directly through the amplifiers of the tape recorder without putting it on tape. The synchronous sensory events could also be presented singly or in combination.

FIGURE 1



Schematic diagram of equipment used to provide delayed or undelayed clicks, light flashes and tactile pulses.

Delayed auditory feedback (DAF). The auditory stimulus consisted of a click of 400 microsec. duration containing frequencies from 500 to 2,000 c.p.s. The intensity was 90 dB. above the threshold of normal hearing. The click was presented through PDR-1 earphones mounted in Wilson No. 258 "sound-barrier" liquid seal enclosures.

Delayed visual feedback (DVF). The visual stimulus was produced by discharging a 16 mfd. capacitor charged to 300 V. into a kemlight electronic flash tube type CX-7. The duration of the flash was 0.2 millisecc. The flash illuminated a 5-in. diameter circle of grey plastic sheeting which was placed 20 cm. in front of the subject and on a level with his eyes. The flashes were discharged in a dark room. There was a soft "click" associated with each discharge of the bulb.

Delayed tactile feedback (DTF). The tactile stimulus was produced by energizing a thyratron circuit which was used to discharge a solenoid. A hard fibre dowel was attached to the solenoid, and placed $\frac{1}{2}$ in. above the subject's forearm. Each time the subject tapped on the key, the solenoid discharged and the dowel hit a spot on the subject's forearm. The impulse delivered by the solenoid was 820 gm.-cm. per sec. as determined by ballistic measurement. The mechanical sounds produced by the operation of the solenoid were masked by presenting high-level white noise to the subject through earphones.

Protocol for experiments

General. All subjects were run on the 17 sub-experiments of this study, except for one subject who did not participate in the four conditions in which digital block was used.

The experiments were divided into three groups and each subject was run on the experiments of each group during one session. The experimental sessions for each group were spaced about one week apart. Group I experiments concerned the effects of decreased sensory feedback on the performance of the regularity task. Group II experiments concerned the effects of delayed sensory feedback on both regularity and pattern tasks. Group III experiments were done with digital block of the tapping finger, and concerned the effects of decreased and delayed sensory feedback on the regularity task, and delayed sensory feedback on the pattern task.

Each experimental condition was preceded by a training period and a control condition. If the regularity task was used, the subject trained by tapping 20 times with the metronome and meter operating as described above to give him information about the desired rate and amplitude of tapping. If the pattern task was used, the subject trained by tapping ten groups of three taps each with the metronome and meter operating. After either training session, there was a 30-sec. pause, and the subject was then run on a control condition. This was followed by a 60-sec. pause, and the subject was then run on the experimental condition. This protocol was followed for all experiments except some in which digital block was used. The exceptions in this case will be described in the discussion of Group III experiments below. The control sample and experimental condition sample consisted of 25 taps for the regularity task, and 12 groups of three taps each for the pattern task. Before each condition the subject was instructed to tap in the required fashion until told to stop by the experimenter.

Decreased sensory feedback (Group I experiments)

Experiment I.—Control condition: normal.

Experimental condition: normal.

We use "normal" to mean the normal pattern of sensory feedback which accompanies the keytapping performance. The subject watches his hand as he taps, hears all of the mechanical sound produced by the tapping, and feels all of the normal sensory feedback from the tapping finger and hand.

Experiment II.—Control condition: normal.

Experimental condition: decreased auditory feedback.

Experiment III.—Control condition: normal.

Experimental condition: decreased visual feedback.

Experiment IV.—Control condition: normal.

Experimental condition: vibration.

The subject watched his tapping finger in all experimental conditions except that of decreased visual feedback. The order in which these four experiments were run was randomized across subjects. Only the regularity motor task was used for these experiments.

Delayed sensory feedback (Group II experiments)

Experiment I.—Control condition: synchronous auditory feedback.

Experimental condition: delayed auditory feedback.

Experiment II.—Control condition: synchronous visual feedback.

Experimental condition: delayed visual feedback.

Experiment III.—Control condition: synchronous tactile feedback.

Experimental condition: delayed tactile feedback.

Experiment IV.—Control condition: synchronous auditory, visual, and tactile feedback.

Experimental condition: delayed auditory, visual, and tactile feedback.

The subject watched his tapping finger during all conditions except those of Experiments II and IV, when he was required to keep his eyes fixed on the light source in front of him. The regularity task was used first, and the order in which the four experiments were run was randomized across subjects. The four experiments were then run again using the pattern task and the same random order of presentation used for the regularity task was used again for each subject.

In this study, the words "sensory feedback" are used in discussion of both the conditions of decreased and delayed sensory alterations. This uniformity of terms is used to simplify the discussion of experiments, but there is actually an important difference between the conditions of decreased sensory feedback and the conditions of delayed sensory feedback. In the former case, we were working with the normal pattern of sensory events which accompanies keytapping. We diminished the amount of undistorted sensory information generated by the tapping. This diminution in normal sensory information is accomplished in different ways for the different sensory modalities, as discussed earlier. However, in each case, we are approximating a decrease in sensory information which is a normal accompaniment of the motor performance, and therefore a part of the normal sensory feedback pattern. In the case of the delayed sensory feedback conditions, we are generating sensory events which we link in time to the motor events. These are not real sensory feedback events in so far as they do not normally accompany key tapping. These delayed sensory stimuli are of constant duration and intensity, and therefore provide no information pertinent to the monitoring of amplitude of tapping. They simply bear a fixed time relationship to the tapping, and can, therefore, be used to monitor rate. The important distinction we are making, however, is one between alteration of a sensory event which is a normal accompaniment of the motor activity under study, and an experimentally generated sensory event which is linked in time to the motor performance. Strictly speaking, the term "sensory feedback" is only applicable to the conditions of the present study in which normal sensory accompaniments of the motor activity have been altered. The term "delayed sensory event" would be a more accurate description of the conditions in which temporal alterations of stimuli have been made. However, we have decided upon the use of the term "sensory feedback" for description of all the experimental conditions of this study in order to simplify the presentation. The distinctions just made, however are important, and should be kept clearly in mind.

Digital block (Group III experiments)

Experiment I.—Control condition: normal.

Experimental condition: normal.

The delay between the control and experimental conditions was 15 min.

Experiment II.—Control condition: normal.

Experimental condition: digital block.

Since it took from 5 to 15 min. for the digital block to result in complete loss of light touch and pin prick sensation, the experimental condition was run 15 min. after the control condition. Experiment I was used to determine if this time delay had an independent effect on the performance of the experimental condition of Experiment II.

Experiment III.—Control condition: digital block.

Experimental condition: digital block, and decreased auditory and visual feedback, and vibration.

Experiment IV.—Control condition: digital block and synchronous auditory, visual and tactile feedback.

Experimental condition: digital block and delayed auditory, visual, and tactile feedback.

The subject watched his tapping finger during all conditions except those of Experiment IV, in which he looked at the light source. The regularity task only was used for Experiments I, II, and III. Experiment IV was run first using the regularity task and then again using the pattern task. The experiments of this group were run in fixed order as listed, for all subjects.

Measures

In each experiment, the same number of taps was obtained in the control and experimental conditions. If the regularity task was used, the subject was told to stop tapping after 25 taps had been made. The distance from one tap to the next was measured in millimeters, beginning with the first tap, to give a total of 20 consecutive measurements. Since this distance measure is directly proportional to time, it is referred to as the "unit-to-unit time" measure. However, the distances from tap to tap, in millimeters, were the measures used in the statistical analyses, and are the measures listed in the tables under "unit-to-unit time."

The greatest distance which the galvanometer pen moved from the baseline during each tap was directly proportional to the maximum force exerted on the key during each tap. This measure is referred to as the "intensity" of the tap. The greatest distance which the pen moved from the baseline, in millimeters, was the measure used in the statistical analyses, and are the measures listed in the tables under "intensity." The intensity of 20 consecutive taps was measured, starting with the first tap.

If the pattern task was used, the subject was told to stop tapping after 12 groups of three taps each were made. Twenty consecutive measurements of unit-to-unit time were made for each condition, starting with the first tap. In this case, the unit-to-unit time was the distance in millimeters between one tap and the next within a group. Twenty consecutive intensity measurements were made for each condition, starting with the first tap.

In summary, the raw data for this study consisted of 20 consecutive unit-to-unit time measures and 20 consecutive intensity measures for each control condition, and each experimental condition. The medians of each 20 raw scores were computed and used in the statistical tests.

RESULTS

Control experiments

As noted above, two control experiments were run, to determine whether the time delays between the control and experimental conditions would produce significant changes in tapping. The two conditions of the regularity task with normal sensory feedback which were spaced 60 sec. apart were not significantly different with respect to either unit-to-unit time or intensity (Table 1). The two conditions of pattern task tapping which were spaced 15 min. apart were not significantly different with respect to unit-to-unit time, but the intensity of tapping increased during the later condition ($p < 0.05$).

Decreased sensory feedback

Regularity task. Three of the decreased sensory feedback conditions produced significant changes in unit-to-unit time and intensity of tapping for the regularity task. These conditions were: decreased auditory feedback; vibration; and the combined decreased sensory feedback consisting of decreased auditory and visual feedback, vibration, and digital block of the tapping finger (Table 1). The decreased auditory feedback and vibration conditions resulted in decreases in the unit-to-unit time which were significant at the 0.05 level. The direction of the change is not reflected in the median scores because of considerable scatter in the data. The combined decreased sensory feedback condition resulted, however, in a decreased rate of tapping (increased unit-to-unit time) which was significant at the 0.01 level, and which is clearly reflected in the median scores.

Intensity of tapping increased under each of the three decreased sensory feedback conditions noted above. The decreased auditory feedback condition resulted in an increase in intensity of tapping which was significant at the 0.05 level, but which is not reflected by appreciable change in the median scores because of scatter in the data. The vibration condition and the combined decreased sensory feedback condition both resulted in increases in intensity of tapping which were significant at the 0.001 level.

We wished to determine whether the combined decreased sensory feedback condition produced significantly more change in tapping for the regularity task than any of the component decreased sensory feedback conditions. The experimental condition were compared with comparable difference scores for each of the component decreased sensory feedback conditions. These comparisons are shown in Table II. The combined decreased sensory feedback condition had a significantly

TABLE I

MEAN MEASURES OF UNIT-TO-UNIT TIME AND INTENSITY ON REGULARITY TASK UNDER NORMAL AND DECREASED SENSORY FEEDBACK

Control conditions			Experimental conditions		
Description	Median		Description	Median	p*
Unit-to-unit time					
Normal ..	8.0		Normal	8.0	NS
Normal ..	8.4		Decreased auditory	8.4	<0.05
Normal ..	8.2		Decreased visual	8.4	NS
Normal ..	8.4		Vibration	8.4	<0.05
Normal ..	8.6		Digital block	8.2	NS
Digital block ..	8.2		Decreased auditory and visual feedback, vibration and digital block	9.4	<0.01
Intensity					
Normal ..	3.0		Normal	3.0	NS
Normal ..	2.8		Decreased auditory	2.9	<0.05
Normal ..	2.7		Decreased visual	2.1	NS
Normal ..	2.5		Vibration	3.6	<0.001
Normal ..	2.1		Digital block	2.0	NS
Digital block ..	2.0		Decreased auditory and visual feedback, vibration and digital block	3.0	<0.001

* Wilcoxon's paired replicates test.

greater effect on both unit-to-unit time and intensity of tapping than any of the component decreased sensory feedback conditions with the exception of the vibration condition. The effect of the combined decreased sensory feedback on intensity of tapping was not significantly different from the effect of the vibration condition alone.

TABLE II

COMPARISONS¹ OF THE EFFECTS ON REGULARITY TASK OF COMBINED AND COMPONENT DECREASED SENSORY FEEDBACK CONDITIONS

	Effects of decreased auditory feedback alone	Effects of decreased visual feedback alone	Effects of vibration alone	Effects of digital block alone
Effects of decreased auditory and visual feedback, vibration and digital block ..	<0.01	<0.05	<0.01	<0.05
Unit-to-unit time	<0.05	<0.01	NS	<0.001
Intensity				

¹ Wilcoxon's paired replicates test.
p values are shown for the experimental minus the control condition scores for component decreased sensory feedback conditions compared to experimental minus control condition scores for the combined decreased sensory feedback condition. All significant differences are in the direction of a greater effect for the combined decreased sensory feedback condition.

Delayed sensory feedback

Regularity task. Each of the component conditions of delayed sensory feedback produced a significant change in either unit-to-unit time or intensity of tapping (Table III). The delayed tactile feedback condition resulted in a decrease in unit-to-unit time which was significant at the 0.05 level. The delayed auditory and delayed visual feedback conditions resulted in increases in intensity of tapping which were significant at the 0.05 level. None of these changes is clearly reflected in the median scores because of considerable scatter in the data.

TABLE III
MEAN MEASURES OF UNIT-TO-UNIT TIME AND INTENSITY ON REGULARITY
TASK UNDER SYNCHRONOUS AND DELAYED SENSORY FEEDBACK

Control conditions		Experimental conditions		
Description	Median	Description	Median	p*
Unit- to-unit time				
Synchronous auditory	8.0	Delayed auditory	7.8	NS
Synchronous visual	8.1	Delayed visual	8.4	NS
Synchronous tactile	8.2	Delayed tactile	8.0	NS
Synchronous auditory, visual and tactile	8.0	Delayed auditory, visual and tactile	7.9	NS
Synchronous auditory, visual, tactile and digital block ..	8.2	Delayed auditory, visual, tactile and digital block	8.7	NS
Intensity				
Synchronous auditory	2.2	Delayed auditory	2.4	<0.05
Synchronous visual	2.3	Delayed visual	2.8	<0.05
Synchronous tactile	2.5	Delayed tactile	2.6	NS
Synchronous auditory, visual and tactile	2.0	Delayed auditory, visual and tactile	2.0	NS
Synchronous auditory, visual, tactile and digital block ..	2.0	Delayed auditory, visual, tactile and digital block	2.2	NS

* Wilcoxon's paired replicates test.

In general, the conditions of delayed sensory feedback did not have much effect on tapping for the regularity task. None of the conditions of delayed sensory feedback produced significant changes in both unit-to-unit time and intensity scores.

Neither of the combined delay conditions produced significant changes in either unit-to-unit time or intensity of tapping.

Pattern task. The conditions of delayed sensory feedback had marked effects on the pattern task. Almost all of these conditions resulted in highly significant increases in both unit-to-unit time and intensity of tapping (Table IV). The only exception was the condition of delayed tactile feedback, under which the changes in unit-to-unit time were not significant.

We wished to determine whether the condition of combined delayed auditory, visual and tactile feedback produced more change in tapping for the pattern task than any of the component delayed sensory feedback conditions. The experimental minus the control scores for the combined delayed sensory feedback condition were compared with comparable difference scores for each of the component delayed

TABLE IV

MEDIAN MEASURES OF UNIT-TO-UNIT TIME AND INTENSITY ON PATTERN TASK
UNDER SYNCHRONOUS AND DELAYED SENSORY FEEDBACK

Control conditions		Experimental conditions		
Description	Median	Description	Median	p*
Unit-to-unit time				
Synchronous auditory ..	7.8	Delayed auditory	8.8	<0.001
Synchronous visual	8.2	Delayed visual	8.1	<0.05
Synchronous tactile	8.4	Delayed tactile	8.6	NS
Synchronous auditory, visual and tactile	8.0	Delayed auditory, visual and tactile	9.2	<0.001
Synchronous auditory, visual, tactile and digital block ..	8.2	Delayed auditory, visual, tactile and digital block	9.6	<0.001
Intensity				
Synchronous auditory ..	2.5	Delayed auditory	3.4	<0.001
Synchronous visual	2.8	Delayed visual	3.5	<0.001
Synchronous tactile	2.8	Delayed tactile	3.5	<0.01
Synchronous auditory, visual and tactile	2.2	Delayed auditory, visual and tactile	3.3	<0.001
Synchronous auditory, visual, tactile and digital block ..	2.0	Delayed auditory, visual, tactile and digital block	3.2	<0.001

* Wilcoxon's paired replicates test.

sensory feedback conditions. These comparisons are shown in Table V. The combined delayed sensory feedback condition had a significantly greater effect on both unit-to-unit time and intensity of tapping than any of the component feedback conditions with the exception of the delayed auditory feedback condition. The effect of delayed auditory feedback alone was not significantly different from the effect of the combined delayed sensory feedback condition on these measures.

TABLE V
COMPARISON¹ OF THE EFFECTS ON PATTERN TASK OF COMBINED AND
COMPONENT DELAYED SENSORY FEEDBACK CONDITIONS

	Effects of DAF alone	Effects of DVF alone	Effects of DTF alone
Effects of DAF, DVF and DTF	NS	<0.01	<0.01
Unit-to-unit time	NS	<0.01	<0.01
Intensity			

¹ Wilcoxon's paired replicates test.
p values are shown for the experimental minus the control condition scores for component delayed sensory feedback conditions compared to experimental minus control condition scores for the combined delayed sensory feedback condition. All significant differences are in the direction of a greater effect for the combined delayed sensory feedback condition.

The change in performance due to the combined delay condition was correlated with the change due to each of the component delayed sensory feedback conditions (Table VI). The correlations were computed for difference scores obtained by subtracting the control condition score from the experimental condition score for each of the experimental conditions listed.

TABLE VI
RANK ORDER CORRELATIONS¹ BETWEEN THE EFFECTS ON PATTERN TASK OF
COMBINED AND COMPONENT DELAYS IN SENSORY FEEDBACK CONDITIONS

	<i>DVF alone</i>	<i>DTF alone</i>	<i>DAF, DVF, DTF</i>	<i>DAF, DVF, DTF and digital block²</i>
Unit-to-unit time				
DAF alone ..	0.54*	0.71**	0.68**	0.24
DVF alone ..		0.64**	0.50	0.10
DTF alone ..			0.44	-0.01
DAF, DVF, DTF				0.33
Intensity				
DAF alone ..	0.33	0.00	0.37	0.34
DVF alone ..		0.43	0.69**	0.29
DTF alone ..			0.46	-0.02
DAF, DVF, DTF				0.58*

¹ The correlations shown are for difference scores between conditions listed and their appropriate controls (see text). The control conditions consist of the same sensory feedback without delay.

² $n = 14$ ($n = 15$ for all other conditions).

* $p < 0.05$.

** $p < 0.01$.

$r_s(0.05) = 0.51$ for $n = 15$.

$r_s(0.01) = 0.64$ for $n = 15$.

$r_s(0.05) = 0.53$ for $n = 14$.

$r_s(0.01) = 0.66$ for $n = 14$.

There is a high degree of correlation between the effects of combined and component delayed sensory feedback conditions on the unit-to-unit time of pattern-task tapping. The component conditions also correlate well with each other. However, the effects on unit-to-unit time of the combined delayed sensory feedback condition concomitant with digital block of the tapping finger do not correlate significantly with the effects on unit-to-unit time due to any of the other delayed sensory feedback conditions.

There is little significant correlation between the effects of combined and component delayed sensory feedback conditions on the intensity of pattern-task tapping.

DISCUSSION

Effects of decreased sensory feedback on motor performance

Control of regularity-task performance was altered by two of the four separate decreases in sensory feedback. Elimination of visual feedback and digital block of the tapping finger resulted in no significant change in rate or amplitude of tapping. Decreased auditory feedback and vibration did, however, result in increased intensity

and rate of tapping. It is of interest to note that the two conditions which produced no alteration in performance were actual reductions of sensory feedback, whereas the conditions which produced alterations involved noise or masking. It is possible that the changes observed under the masking conditions might have been due, at least in part, to the masking itself, and not to the decrease in sensory feedback.

Our findings suggest that alterations in proprioceptive feedback result in more marked disturbance of tapping performance than any of the other single alterations in sensory feedback studied. We may speculate that the increase in intensity under the vibration condition might be compensatory, functioning to counteract the disturbance produced by vibratory masking. This possibility may be more critically studied in animals by selective decrease in proprioceptive feedback, obtained by placing a surgical lesion in the posterior columns of the spinal cord.

The condition of combined decreases in sensory feedback resulted in significant alterations in regularity-task tapping performance. Under this condition, there was a significant increase in intensity of tapping and a significant decrease in rate of tapping. The increase in intensity of tapping due to the combined decreased sensory feedback condition was not significantly different from the increase in intensity of tapping due to vibration alone. It follows, that if any of the other decreased sensory feedback conditions contribute to the increase in intensity of tapping, their contribution does not add significantly to the increased intensity due to vibration alone. These findings suggest that proprioception may be the most important type of sensory feedback required to control the intensity of tapping in these experiments.

The decrease in rate of tapping which occurs under the combined condition is not observed in any of the component conditions of decreased sensory feedback. The two component conditions which do affect rate of tapping result in an increased rate (Table I). The decreased rate of tapping under the combined condition is, therefore, unique to this condition, and indicates that the combination of all of the separate decreases in sensory feedback have effects on tapping which are not observed when any one of them is used alone. Under the combined condition, many of the subjects reported that they had no knowledge of whether they were contacting the key. It would be interesting to do experiments in which the different combinations of two and three conditions of decreased sensory feedback were used separately. One could then determine the specific combinations of conditions which result in the marked decrease in tapping rate observed when all four conditions of decreased sensory feedback were used together. Perhaps it is necessary to decrease all four modalities of sensory feedback simultaneously to produce the marked decrease in tapping rate. There is considerable evidence that there is a wide safety margin in the amount of sensory information which must return to the central nervous system in order to maintain control over voluntary motor activity of limbs. Sherrington showed that sectioning of all of the dorsal roots of the brachial plexus in the monkey results in an almost complete inability to use the deafferented limb (Sherrington and Mott, 1894-95). However, if the dorsal root of C, alone is left intact, the resulting voluntary motor deficit is minimal (Lassek, 1953). These findings indicate that far more sensory feedback returns to the central nervous system from an extremity than is needed for monitoring its voluntary motor activity. Since each dorsal root carries several sensory modalities, it is clear that in the case in which only one dorsal root is spared, sensory feedback from a number of sensory receptors is still present. The relative importance of the different types of sensory feedback needed for control of voluntary movement depends on the parameter of motor control in question. When we consider the problem of monitoring the rate of tapping, no one type of sensory feedback seems to provide more sensitive information about this

aspect of performance than any other. If this is the case, then marked changes in rate would only be expected to follow upon decreases in all of the types of sensory feedback, as was observed to be the case. For the control of intensity of tapping, however, our findings suggest that proprioception may be more important than the other types of sensory feedback studied. It would seem that proprioceptive feedback would show more variation as a function of changes in intensity of tapping than would visual or auditory feedback.

The relative importance of a specific type of sensory feedback for control of voluntary movement also varies with the total pattern of sensory feedback of which it is a part, and therefore, with the motor performance being controlled. Provins (1955) had human subjects reproduce pressures with their index fingers under a condition in which the digital nerves were blocked, and under a condition without anesthesia. Subjects became more accurate in their performance with anesthesia when they were reproducing pressures over 400 gm. The lower the pressure being reproduced, the more difference there was between the performance with and without anesthesia. Provins implies that below 400 gm., optimal motor control depends more upon touch receptors than upon those in muscles and tendon. We do not normally require visual information about the location of our feet in order to maintain balance; however, as proprioceptive information from the legs is decreased, the visual feedback becomes progressively more essential.

Effects of delayed sensory feedback on motor performance

The conditions of delayed sensory feedback did not markedly alter performance of the regularity task as indicated by the following: none of the delayed sensory feedback conditions produced significant changes beyond the 0.05 level; neither of the combined delayed sensory feedback conditions produced significant changes in tapping (Table III).

The same conditions of delayed sensory feedback did produce marked changes in the performance of the pattern task as indicated by the following: all conditions of delayed sensory feedback, except the delayed tactile feedback condition, produce significant changes in both intensity and rate; most of the changes produced were significant at better than the 0.001 level, both of the combined delayed sensory feedback conditions produced changes in intensity and rate which were significant at better than the 0.001 level (Table IV). All of the significant changes in intensity and rate of tapping were in the directions previously reported to result under conditions of delayed sensory feedback: intensity always increased, and rate always decreased (Chase, Harvey, Standfast, Rapin and Sutton, 1959).

The fact that the pattern task is more disturbed by delayed sensory feedback than the regularity task suggests that motor events which are more complex in time are more easily disturbed by delayed sensory feedback. In recent studies, we tried to determine how close to the threshold of normal hearing a delayed click could be and still produce definite changes in keytapping performance (Chase, Sutton, Fowler, Fay and Ruhm, 1961). We found that a click 10 dB. above the threshold of normal hearing produced little change in regularity-task tapping, but produced more change when the subject tapped in groups of three taps each. However, it was not until subjects were required to tap a still more complex pattern (. . . .) that the 10 dB. delayed click produced significant changes in the rate of tapping for the majority of subjects. This finding provides further support for the idea that motor events which are more complex in time are more easily disturbed by delayed sensory feedback.

We have previously reported that delayed auditory feedback results in increased intensity and decreased rate of keytapping (Chase, Harvey, Standfast, Rapin and

Sutton, 1959). In the present study we observe that delayed light flashes and delayed tactile pulses produce the same qualitative changes in tapping (Table IV). It may be objected that the low-intensity click produced by discharge of the electronic flash tube was responsible for the changes in tapping under this condition. However, we have repeated this experiment using ear enclosures which mask the click entirely, and subjects still show increased intensity and decreased rate of tapping when they are exposed to delayed light flashes. The delayed tactile pulses did not produce significant changes in rate of tapping; however, decreased rate of tapping was often observed under this condition. We feel that all of the delayed sensory feedback conditions studied produce the same qualitative changes in keytapping: increases in intensity and decreases in rate. There was also a tendency to tap four times instead of three under all delayed sensory feedback conditions. Since the relative intensities of the delayed sensory stimuli were not controlled, we could not compare the relative amounts of change in tapping resulting from each of the delayed feedback conditions.

There is normally some auditory feedback associated with keytapping, and it might be assumed that this type of sensory feedback normally plays some rôle in learning this type of motor activity. However, light flashes and tactile pulses on the forearm are sensory events which are not usually part of the pattern of sensory feedback associated with keytapping. It would seem that the nervous system is not able to separate the delayed tactile and visual sensory input from the normal pattern of sensory feedback used to monitor keytapping. The ability of such a wide variety of delayed sensory events to disturb pattern-tapping performance suggests that the sensory feedback systems used for monitoring temporally complex motor events are easily disturbed by sensory events of any type which are displaced in time from the motor events being monitored.

There are significant correlations between the effects of all the separate delayed sensory feedback conditions on the rate of pattern-task tapping (Table VI). It would seem that some individuals are more vulnerable to the disturbing influences of delayed sensory feedback on keytapping regardless of the modality of the delayed sensory event.

The original studies which demonstrated the disturbing influence of delayed sensory feedback on voluntary motor control were done on the delayed auditory feedback influences on speech. The studies on this feedback system reveal three major changes in speech under delayed auditory feedback: (a) increase in mean vocal intensity, (b) increase in phonation time, and (c) a tendency to repeat parts of words. In a previous study, we wished to determine whether auditory feedback would have the same type of effect on different types of motor performance (Chase, Harvey, Standfast, Rapin and Sutton, 1961). A population of normal subjects performed a speech task and a keytapping task under similar conditions of delayed auditory feedback. We found that the changes in keytapping under delayed auditory feedback were qualitatively the same as those produced in speech under delayed auditory feedback. The subjects tapped the key harder, held the key down longer, and tended to tap four times instead of three. We concluded that delayed auditory feedback can produce similar effects on widely different motor performances. We have just noted above that a wide variety of delayed sensory events can produce similar effects on the same motor performance.

Mechanism of motor disturbance caused by delayed sensory feedback

By what mechanism might the delayed sensory events disturb the monitoring of voluntary motor activity? The delayed sensory events frequently overlap in time with the successive units of motor performance. Since the normal pattern of sensory

feedback used for monitoring occurs simultaneously with motor performance, the disturbance in motor performance might be considered due to the distortion in the normal pattern of sensory feedback information utilized for monitoring. It is also possible that the delayed sensory events disturb voluntary motor activity by mechanisms which are independent of distortion of normal sensory feedback patterns. It is possible for example, that the delayed sensory event is "utilized" by the central nervous system as if it were part of the normal sensory feedback pattern. The delayed sensory event would then be interpreted as indicating a delay in motor performance, and a correction of motor response appropriate to such a delay would then result. Since the delayed sensory event is actually not a correct indication of the timing of the motor response, the resulting "correction" would result in a disturbance of motor activity. The first mechanism noted above could be examined by doing experiments on keytapping in which the delayed sensory events were never permitted to overlap in time with a motor response. In such an experiment, the delayed sensory events could not possibly distort the normal sensory feedback pattern, and any disturbance in motor performance which resulted would have to be due to some other mechanism. We have discussed at greater length elsewhere the possible mechanisms by which delayed sensory events might disturb voluntary motor activity (Chase, and Guilfoyle, 1961).

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REFERENCES

- ADRIAN, E. D. (1947). General principles of nervous activity. *Brain*, **70**, 1-17.
- BASTIAN, H. C. (1909). The functions of the kinaesthetic area of the brain. *Brain*, **32**, 327-41.
- CHASE, R. A. (1958). Effect of delayed auditory feedback on the repetition of speech sounds. *J. Speech Dis.*, **23**, 583-90.
- CHASE, R. A., HARVEY, S., STANDFAST, S., RAPIN, I. and SUTTON, S. (1959). Comparison of the effects of delayed auditory feedback on speech and keytapping. *Science*, **129**, 903-4.
- CHASE, R. A., SUTTON, S., and FIRST, D. (1961). A developmental study of changes in behaviour under delayed auditory feedback. *J. genet. Psychol.* (in press).
- CHASE, R. A., HARVEY, S., STANDFAST, S., RAPIN, I., and SUTTON, S. (1961). Studies on sensory feedback I: Effect of delayed auditory feedback on speech and keytapping. *Quart. J. exp. Psychol.*, **13**, 141-52.
- CHASE, R. A., SUTTON, S., FOWLER, E. P. JR., FAY, T. H., and RUHM, H. B. (1961). The effect of low sensation level delayed clicks on keytapping. *J. Speech hearing Res.* (in press).
- CHASE, R. A., and GUILFOYLE, G. (1961). The effect of simultaneous delayed and undelayed auditory feedback on speech. *J. Speech hearing Res.* (in press).
- DENNY-BROWN, D. (1950). Disintegration of motor function resulting from cerebral lesions. *J. nerv. ment. Dis.*, **112**, 1-45.
- FITTS, P. M. (1951). Engineering psychology and equipment design. In S. S. Stevens (Ed.), *Handbook of Experimental Psychology*, New York. Pp. 1287-340.
- GOODY, W. (1949). Sensation and volition. *Brain*, **72**, 312-39.
- JACKSON, J. H. (1958). *Selected Writings of John Hughlings Jackson*, Vol. 2. New York. Pp. 201, 400-1.
- KNAPP, H. D., TAUB, E., and BERMAN, A. J. (1958). Effect of deafferentation on a conditioned avoidance response. *Science*, **128**, 842-3.

- LASSEK, A. M. (1953). Potency of isolated brachial dorsal roots in controlling muscular physiology. *Neurology*, **3**, 53-7.
- LASSEK, A. M. (1954). *The Pyramidal Tract*. Illinois. Pp. 76-90.
- MELTON, A. W., and BRIGGS, C. E. (1960). Engineering Psychology. *Annual Review of Psychology*, **11**, Palo Alto. Pp. 71-98.
- PROVINS, K. A. (1955). The rôle of receptors in muscle and tendon in controlling the application of finger pressure in man. *J. Physiol.*, **128**, 55-6.
- RUCH, T. C. (1951). Motor Systems. In S. S. Stevens (Ed.), *Handbook of Experimental Psychology*. New York. Pp. 154-208.
- SHERRINGTON, C. S., and MOTT, F. W. (1894-95). The influence of sensory nerves upon the movement and nutrition of the limbs. *Proc. roy. Soc.*, **57**, 481-8. Reprinted in part in D. Denny-Brown (Ed.) (1940), *Selected Writings of Sir Charles Sherrington*. New York. Pp. 115-9.
- SHERRINGTON, C. (1952). *The Integrative Action of the Nervous System*. London.
- TWITCHELL, T. E. (1954). Sensory factors in purposive movement. *J. Neurophysiol.*, **17**, 239-52.
- WALSHE, F. M. R. (1947). On the rôle of the pyramidal system in willed movement. *Brain*, **70**, 329-54.

SHORTER ARTICLES AND NOTES

ON KNOWING WITH WHICH EYE ONE IS SEEING

BY

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Experiments were performed to determine whether it is possible to discriminate which eye or eyes are being stimulated by a point source of light in a dark room when the subject is unaware which eye, if any, is occluded.

There were two stimulus conditions, (a) a continuous source of light, and (b) a flash. The results show that in these conditions discrimination is faulty and, in some instances, at chance level, although there is considerable individual variation. The implications of these results for the argument from transfer, as applied to after-effects, and the theory of depth perception are considered.

INTRODUCTION

Is it possible to tell if one is seeing with one eye or both, and, if one eye alone, which eye it is? If it is argued that the response of "seeing" involves the whole organism, then the question may be reformulated as that of knowing which eye, or eyes are being stimulated.

This question is discussed by Helmholtz (1925). It is distinguished from the effect of transposing each retinal image to the corresponding points of the other eye, which does result in a detectable difference. Helmholtz points out that our judgments are influenced by experience, to the extent that objects in a part of the field normally seen by one eye only are always judged to be viewed by that eye, even when they are not, and he concludes, although giving no further evidence, that "without making a special experiment for that very purpose, we are ignorant as to which image belongs to one eye, and which to the other eye" (p. 459).

In his notes on the chapter, von Kries quotes experiments by Brückner and von Brücke which show mistakes can be made and particularly when both eyes are looking through separate black tubes whose apertures are fused stereoscopically (Helmholtz, 1925, p. 492). However, these authors reject, for other and invalid reasons, the conclusion that discrimination is impossible.

In normal conditions, the use of one eye results from the voluntary closing of the other, implying that the subject knows what is the state of affairs. Even if he were unaware that one eye was occluded, the unusual, and reduced visual field might lead him to suspect that this was the case.

However, the question whether the discrimination could be made in the absence of information from either source, i.e. voluntary muscular control, or characteristic visual field, seemed still to be an open one, and the following experiments were designed as a preliminary attempt to answer it.

EXPERIMENT I

Apparatus and Method

The subjects sat about 4 ft. from a point source of light, which was about 2 ft. above the line of regard, in a totally dark room. The light, which was only just visible, remained on continuously. A pair of optician's test frames was fitted onto the subjects in the dark before each of the 20 trials. A cardboard "blank" could be put in place, allowing the

right, left or both eyes to remain unoccluded. The order of trials was randomized, but was the same for each subject. The following instructions were given:

"I am going to put this pair of spectacles on you and ask you whether you see that spot of light with one eye or both. I just want to know what you feel you see, so please don't try to find out the answer by closing one eye or moving your head, because that will spoil the purpose of the experiment. Don't try to adjust the spectacles yourself and please close your eyes while I am putting them on you."

If a subject was myopic it was ascertained if the image appeared different to the two eyes.

Subjects

Twelve subjects were tested, all but one being students of psychology.

Results

The number of errors made by each subject is shown in Table I.

TABLE I
DISTRIBUTION OF ERRORS BY SUBJECTS

Subject ..	1	2	3	4	5	6	7	8	9	10	11	12
No. of errors ..	0	1	1	3	5	6	6	7	10	10	12	12

The total number of errors is 73, and the average number is 6, and as there was a 1 in 3 chance of guessing right the group as a whole is performing at better than chance level. However, the results suggest a considerable degree of uncertainty, and the last four subjects in particular are very little better than would be expected by chance (approximately 14 errors).

Subjects 5, 8 and 11 admitted to different images in the two eyes. Subjects 5 and 8 were myopic and did not have their spectacles; the image in one eye was more blurred than in the other. Subject 11 had a "lazy" right eye and also felt the images were very different in the two eyes. In spite of this their error scores are comparatively high.

The distribution of wrong judgements for each of the three conditions of stimulation is shown in Table II.

TABLE II
DISTRIBUTION OF ERRORS BY EYE STIMULATED

Eye stimulated	Eye to which stimulation referred			Total errors
	R	L	R and L	
R	—	3	28	31
L	8	12	13	21
R and L ..	9	—	—	21

When using both eyes errors of judgement were fairly evenly distributed between the right and left eyes. Subjects using one eye, however, were nearly four times as likely to think that they were using both eyes as that they were using the other eye. No evidence of learning is shown, there being as many errors in the first 10 trials as the second.

Individual feelings of confidence varied greatly and bore little relation to performance. Some subjects said they could "feel" when there was a patch in front of one eye, although they could not see it.

Discussion

There is evidence that it is not obvious which eye is responding to a stimulus when both eyes are open and the visual field has no boundaries.

However, overall performance is at better than chance level, and there are also considerable individual differences; these cannot be due to defects of vision assisting the differentiation as subjects with this assistance were, on the whole, worse than average. However, subjects were permitted to inspect the stimulus for as long as they wished before they made their judgement, and the temptation to move the head slightly or to blink must have been great, especially for those subjects who expressed anxiety about the difficulty of making a decision.

A suggestion made by Mr. R. Gregory (personal communication, May, 1959) was adopted, namely that the experiment should be repeated with a very short stimulus duration, thus preventing the subject from "cheating."

EXPERIMENT 2

Method

The previous experiment was repeated, with certain modifications. The stimulus was a point flash of light, lasting 0.012 sec. It was presented in an apparatus designed for the study of apparent movement. The subject sat about 1 ft. back from the eye-piece in order to avoid knocking it with the test frames. The instructions were as before except that the subject was told he would see a brief flash of light. He was warned when it was about to occur by the experimenter saying, "Ready." The order and number of trials was the same as before.

Subjects

There were eight subjects, who were students or staff in the Psychology Department.

Results

The number of errors for each subject is shown in Table III.

TABLE III
DISTRIBUTION OF ERRORS BY SUBJECTS

Subject ..	1	2	3	4	5	6	7	8
No. of errors ..	5	5	7	8	8	9	10	14

The total number of errors is 66, and the average is now 8, compared with 6 in the last experiment. The same continuously graded distribution of errors is found.

Table IV shows the distribution of wrong judgements for the three conditions of stimulation.

TABLE IV
DISTRIBUTION OF ERRORS BY EYE STIMULATED

Eye stimulated	Eye to which stimulation referred			Total errors
	R	L	R and L	
R	—	6	12	18
L	14	—	11	25
R and L ..	14	9	—	23

The tendency to ascribe stimuli to the right eye rather than the left is more pronounced in this experiment; it may be due to a tendency to ascribe effects to the dominant eye.

Subjects 2 and 7 thought the images in the two eyes were a little different. Nevertheless Subject 7's error score is still not amongst the lowest.

In this experiment subjects tended to complain more that they found the task difficult, only three saying they felt reasonably sure of their judgements; again these were not the ones with the lowest scores. Questioning revealed the fact that most subjects were making use of any possible extra cues. Some noticed that the point source of light appeared in different positions on different occasions and tried to deduce from that which eye was covered; one confessed half-way through the experiment that he thought he had been making the wrong deduction. Others tried to hear which side of the test frames contained the cover. The observation that a black patch could sometimes be "felt" in front of one or the other eyes was again made.

Discussion

The number of errors in this experiment is greater than in the preceding one, but not as much greater as might have been expected if judgements were completely random. However, some subjects were making successful use of information from apparent position of the stimulus and possibly from other sources. The experiment was carried out in a different laboratory from before, and a small amount of light entered by a window which might just possibly have helped the subjects to discriminate the eye cover when they became dark adapted. There is in fact a greater number of errors in the first half of this experiment than the second, (39 compared with 27) suggesting that performance was improving, and this was not found in the first experiment. It remains possible that if the experimental conditions were improved, discrimination would drop to chance level.

CONCLUSIONS AND IMPLICATIONS

It has been demonstrated in this experiment and the preceding one that mistakes can be made about which eye is being stimulated, particularly when the stimulus is applied to only one eye. Of the total number of responses (280) made to stimulations to the right and left eyes separately in the two experiments, almost exactly one third are in error.

This finding has implications for an argument in common use in psychological literature, i.e. the argument from transfer. If one eye alone is stimulated, and the subject claims to see an after-effect with the other eye alone, this is usually regarded as evidence that the effect is central. However, the subject's claim to see the after-effect in the unstimulated eye is irrelevant if he is in fact unable to discriminate which eye is being stimulated, and may more easily be ascribed to the influence of past experience, which teaches that what is seen is seen with an open eye. The shape of the visual field would of course have no bearing on the discrimination of the origin of the after-image. For the report of transfer to have any meaning, it would therefore have to occur with at least a reliably greater frequency than that at which errors of discrimination are made, and this has been shown not to be so (Pickersgill, 1959). The question of whether an after-effect is purely central, or whether it is also dependent on retinal processes, must therefore be solved by other means.

The results also have a bearing on the importance of one type of cue usually considered to be playing a considerable part in depth perception, i.e. the stimulation of points which are noncorresponding in the two retinae with respect to the fixation

point. If such information is to have value in localizing the stimulus in space, the ability not to confuse the source of the discrepant stimulations must be presupposed, and in the light of the above results such a presupposition appears of very doubtful validity.

Of course it may be that while one cannot say accurately which eye is being stimulated, this information is in fact available to the nervous system at a lower level. One cannot for instance say which semi-circular canal is being stimulated by a given change in head or body movement, but the effect may all the same be *used* to correct body imbalance. Helmholtz asserts that while the difference between the two retinal images is normally imperceptible, it is nevertheless used in estimating comparative distance, and it thus remains a possibility that disparity cues are used in depth perception in spite of the fact that subjects cannot discriminate at a conscious level which eye is the source of information.

However, there is in fact evidence that disparity cues alone are not very effective: Vernon (1952) summarizes the evidence as indicating that such cues alone can only give rise to knowledge of non-equidistance, and that even this may not be achieved if these cues are in conflict with others. Further information seems necessary for judgements of "nearness" and "farness"; in particular, if all knowledge of the surroundings is absent, as was the case in the present experiments, then depth localization is found to be extremely difficult, even though disparity cues are present.

The author is greatly indebted to Professor M. A. Jeeves, under whose supervision these experiments were carried out, to Professor G. P. Meredith for providing laboratory facilities, and to Mr. Richard Gregory for helpful advice and discussion.

REFERENCES

- HELMHOLTZ, H. von (1925). *Treatise on Physiological Optics*, Vol. III, ed. J. P. C. Southall. The Optical Society of America, U.S.A.
- PICKERSGILL, M. J. (1959). *The Determinants of Perception of some Aspects of Visual Movement and their Relation to certain Personality Variables and to Brain Injury*. Unpublished Ph.D. thesis, University of Leeds.
- VERNON, M. D. (1952). *A Further Study of Visual Perception*. London.

HANDEDNESS, EYE-DOMINANCE, AND IMMEDIATE MEMORY

BY

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Twenty male university students varying in handedness and eye-dominance recalled conventional and unconventional digits under two conditions: (1) when the same *type* of digit was projected simultaneously to each eye, and (2) when a different type of digit was projected simultaneously to each eye.

Forty trials were given, each consisting of four pairs of digits with a different numeral projected to each eye. Performance was analysed in terms of percentage recalled correctly and time of onset of each response.

Right eye-dominant groups, particularly a right handed and right eye-dominant group, were superior in both accuracy of recall and speed of response to left eye-dominant groups. Least able in recall were left handed, left eye-dominant subjects. Superiority of right eye-dominant groups was especially marked when a different type of digit was projected to each eye.

In accord with results reported by Broadbent for the ears (1956, 1957), other results suggest it is possible for the eyes to operate simultaneously as independent information sources. However, unlike the case for audition, present subjects did not report all information from one eye before that from the other, but gave it in pairs. It seems that the capacity of the eyes to function independently depended upon eye-dominance and handedness, while serial ordering of responses reflected imposition of an habitual response organization.

INTRODUCTION

Broadbent (1956, 1957, 1958) has demonstrated for several combinations of sensory modalities that if information is simultaneously presented to two sensory channels, individuals typically report all from one channel before any from the other. He has also explored (1958) implications of this finding for theories of immediate memory. It would seem essential therefore that this mode of behaviour be shown to be characteristic of response to visual stimuli alone, because of the dominance of vision in man's behaviour. This was one reason for the present experiment.

Recent neurophysiological observations indicate that the temporal lobes may play an important rôle in memory (Penfield, 1958). Further, Milner's psychological examination (1958) of temporal-lobe patients has suggested that the dominant lobe may be primarily associated with verbal memory, whereas the minor temporal lobe may be concerned with rapid visual identification. Such observations suggest the possibility of demonstrating a relation between handedness, eye-dominance and immediate memory in the "intact" subject. This was the second aim of the present experiment.

Accordingly, four experimental groups differing in handedness and eye-dominance recalled conventional and unconventional digits under experimental conditions that bisected the visual field and projected different digits simultaneously to each eye. Answers were sought to two specific questions: (1) Can the eyes function simultaneously as independent information sources despite overlap in their central neural organizations? (2) Is speed and accuracy of recall related to handedness and eye-dominance?

METHOD

Subjects. Twenty male university students were separated into four equal groups on the basis of handedness (determined by questioning) and eye-dominance (determined by the finger-aiming test). These groups were designated: RR (right-handed and right eye-dominant), RL (right-handed and left eye-dominant), LR (left-handed and right eye-dominant), and LL (left-handed and left eye-dominant) respectively.

Apparatus. A magazine-load automatic projector was connected to two interval timers which were in turn linked to a milliammeter. These arrangements permitted control of stimulus duration and intervals between successive stimuli. These intervals were recorded on the milliammeter by a chronograph pen attachment.

A standard tape recorder was used for the subject's verbal responses, the timing of which were recorded on the milliammeter by means of a throat microphone connected to it via an amplifier.

A binocular viewer, 3 ft. 6 in. $\times$ 6 in. $\times$ 3 in., was painted matt black, with a centre division down its length that effectively bisected the visual field. There was a day-view screen across the end of the viewer upon which stimuli for each eye were projected.

Conventional and unconventional digits (Lansdell, 1954; Foley, 1956) were used as visual stimuli. These were mounted on 2 in. $\times$ 2 in. glass slides, in pairs, in three combinations as follows:

- (a) 20 random pairs of conventional digits ranging from 0 to 9;
- (b) 10 random pairs of unconventional digits from 2 to 9;
- (c) 12 pairs composed of one conventional and one unconventional digit. Trials in which both types of digits were used on each slide were so arranged that each time either a conventional or unconventional digit appeared on the left it was balanced by the same digit on the right usually in a different trial. The projected size of each stimulus was $\frac{5}{8}$ in.

Procedure. In a pre-experimental session each subject encountered a random series of 10 unconventional digits ranging from 1 to 9 that were projected twice to each eye in turn and identified by the experimenter. The timing of these presentations was variable. Two further presentations to each eye followed in which the subject was required to identify the digits.

In the experiment proper that followed immediately, each subject was given 40 trials. Each trial consisted of 8 digits projected in 4 pairs, one digit to each eye. The digits composing each trial were pre-determined, and particular trials then selected at random in advance for presentation to each subject. The duration of each stimulus pair was 0.3 sec. and the interval from the end of each pair of stimuli to the onset of the next pair was 0.9 sec. Inter-trial intervals were variable but of the order of 60 sec.

The subject was instructed that after each trial he was to call out all the digits he could remember, in any order, as soon as possible after a red 0.3 sec. flash on the screen that followed 0.9 sec. after the fourth pair of digits and terminated every trial.

There were two experimental conditions: (1) one in which the same type of digit was projected simultaneously to each eye, and (2) one in which a different type of digit was projected simultaneously to each eye. The 40 trials were as follows: (a) a different conventional digit projected to each eye (10 trials); (b) a different one of the unconventional digits projected to each eye (10 trials); (c) a conventional digit projected to one eye and a different unconventional one to the other eye (20 trials). In (c), the trials were so arranged that the same number (40) of conventional and unconventional digits were projected to each eye as in (a) and (b) combined.

RESULTS

These data were transformed by the formula: $\text{Angle} = \text{Arcsin } \sqrt{\text{Percentage before analysis}}$.

Performance was assessed in terms of speed (mean time of onset of each response i.e. the mean interval between onset of one response and the onset of the next response) and accuracy (percentage of digits recalled correctly). The speed index excluded the first response on each trial because its average time was inordinately long compared with other responses on a trial and in consequence was thought likely to reflect rehearsal and organization time for all digits retained in memory rather than

timing of the first response alone. It was therefore treated as a separate response category.

When the same *type* of digit was projected to each eye, conventional digits were recalled more accurately (83.4 to 67.5 per cent.) and more rapidly (0.54 to 0.76 sec.) on the average than unconventional ones. On the other hand when a different type of digit was projected to each eye, conventional ones were still recalled more accurately than unconventional ones (81.1 to 76.4 per cent.), but both types were reported at the same speed ($\bar{X} = 0.63$ sec.). The differences between conventional and unconventional digits were statistically significant (Tables I, II, III and IV).

TABLE I

ANALYSIS OF VARIANCE OF PERCENTAGE OF DIGITS RECALLED CORRECTLY WHEN THE SAME TYPE OF DIGIT WAS PROJECTED TO EACH EYE

Source of variation	SS	df	MS	F
Groups (A)	560.0	3	186.7	1.39 (n.s.)
Subjects in the same group ..	2149.5	16	134.3	
Total SS between subjects ..	2709.5	19		
Recall for eyes (B)	0.1	1	0.1	50.77***
Recall for digits (C)	2391.5	1	2391.5	
A × B	582.5	3	194.2	4.12*
A × C	46.5	3	15.5	—
B × C	8.8	1	8.8	—
A × B × C	24.7	3	8.2	—
Pooled subjects × conditions..	2260.9	48	47.1	
Total within Subjects	5315.0	60		
Total SS	8024.5	79		

* Significant beyond the 0.05 level of confidence.

*** Significant beyond the 0.001 level of confidence.

TABLE IA

PERCENTAGE OF DIGITS RECALLED CORRECTLY WHEN THE SAME TYPE OF DIGIT WAS PROJECTED TO EACH EYE

Group	Eye to which digits were projected	
	Left eye	Right eye
LL	78.1	64.3
RR	77.8	85.4
LR	76.6	77.1
RL	71.4	75.6

There was no significant difference in percentage of digits recalled correctly of those projected to each eye (Table I), but there was a highly significant difference in mean speed of response for digits associated with each eye ($p < 0.001$, Table II). Digits projected to the right eye were reported more rapidly than those projected at the same time to the left eye. Further, both performance indices disclosed a

significant Eyes $\times$ Groups interaction ($p < 0.05$, Tables I and II). This interaction associates both speed and accuracy of response with handedness and eye-dominance to an extent contingent upon the eye to which a digit was projected.

TABLE II
ANALYSIS OF VARIANCE OF MEAN RESPONSE ONSET TIMES WHEN THE
SAME TYPE OF DIGIT WAS PROJECTED TO EACH EYE

Source of variation	SS	df	MS	F
Groups (A)	9531.8	3	3177.3	2.27 (n.s.)
Subjects in the same group ..	22440.4	16	1402.5	
Total between subjects ..	31972.2	19		
Recall for eyes (B)	16875.2	1	16875.2	46.69***
Recall for digits (C)	10037.4	1	10037.4	27.77***
A $\times$ B	3691.2	3	1230.4	3.40*
A $\times$ C	223.8	3	74.6	—
B $\times$ C	905.2	1	905.2	2.50 (n.s.)
A $\times$ B $\times$ C	2136.1	3	712.0	1.97 (n.s.)
Pooled subjects $\times$ conditions..	17345.5	48	361.4	
Total within subjects	51214.4	60		
Total SS	83186.6	79		

* Significant beyond the 0.05 level of confidence.

*** Significant beyond the 0.001 level of confidence.

TABLE IIA
MEAN TIMES FOR ONSET OF RESPONSES WHEN THE SAME TYPE OF DIGIT
WAS PROJECTED TO EACH EYE

Group	Eye to which digits were projected	
	Left eye (sec.)	Right eye (sec.)
RR	0.60	0.37
LR	0.76	0.49
LL	0.83	0.69
RL	0.99	0.47

A more detailed inspection of these interactions (Tables IA and IIA) shows that differences in accuracy were less marked for recall of digits projected to the left eye than for recall of digits projected to the right eye (Table IA). Accuracy of recall on the left was very similar in all groups. The greatest difference in accuracy was between groups extreme in *both* handedness and eye-dominance (LL vs. RR) in recall of digits projected to the *right eye*, the LL group recalling fewer (64.3 to 85.4 per cent.) on the right than the RR group. Remaining groups were intermediate in accuracy of recall. Their performances suggest that being right eye-dominant aided recall in the LR group while being left eye-dominant in some manner hindered recall in the RL group. The RR group was also faster than the LL group (Table IIA) in response to digits from both left and right eyes. These differences were not so confined to the right as were differences in accuracy. The disturbance in accuracy of performance resulting

TABLE III

ANALYSIS OF VARIANCE OF PERCENTAGE OF DIGITS RECALLED CORRECTLY
WHEN A DIFFERENT TYPE OF DIGIT WAS PROJECTED TO EACH EYE

Source of Variation	SS	df	MS	F
Groups (A)	1433.5	3	477.8	4.84*
Subjects in the same group ..	1582.2	16	98.8	
Total between subjects ..	3015.7	19		
Recall for digits (B)	214.8	1	214.8	4.48*
Recall for eyes (C)	61.1	1	61.1	1.27 (n.s.)
A × B	41.3	3	13.8	—
A × C	186.9	3	62.3	1.30 (n.s.)
B × C	16.1	1	16.1	—
A × B × C	82.5	3	27.5	—
Pooled subjects × conditions ..	2302.0	48	48.0	
Total within subjects	2904.7	60		
Total SS	5920.4	79		

* Significant beyond the 0.05 level of confidence.

TABLE IIIA

PERCENTAGE OF DIGITS RECALLED CORRECTLY WHEN A DIFFERENT
TYPE OF DIGIT WAS PROJECTED TO EACH EYE

Group	Conventional digits	Unconventional digits
RR	88.3	85.8
LR	81.2	77.6
RL	81.0	72.5
LL	72.7	68.4

TABLE IV

ANALYSIS OF VARIANCE OF MEAN RESPONSE ONSET TIMES WHEN A
DIFFERENT TYPE OF DIGIT WAS PROJECTED TO EACH EYE

Source of variation	SS	df	MS	F
Groups (A)	11269.2	3	3756.4	3.25*
Subjects in same group ..	18508.1	16	1156.8	
Total between subjects ..	29777.3	19		
Recall for digits (B)	32.8	1	32.8	—
Recall for eyes (C)	11433.8	1	11433.8	38.14***
A × B	157.5	3	52.5	—
A × C	512.3	3	170.8	—
B × C	33.3	1	33.3	—
A × B × C	612.9	3	204.3	—
Pooled subjects × conditions ..	14389.7	48	299.8	
Total within subjects	27172.3	60		
Total SS	56949.6	79		

* Significant beyond the 0.05 level of confidence.

*** Significant beyond the 0.001 level of confidence.

TABLE IVA

MEAN TIMES FOR ONSET OF RESPONSES WHEN A DIFFERENT TYPE OF DIGIT WAS PROJECTED TO EACH EYE

Group			Eye to which digits were projected	
			Left eye (sec.)	Right eye (sec.)
RR	..	..	0.52	0.35
LR	..	..	0.75	0.52
RL	..	..	0.83	0.52
LL	..	..	0.88	0.64

from being left eye-dominant in the RL group when its performance was contrasted with that of the RR group, was here even more marked in speed of response (Table IIA), especially as it was slower even than the LL group.

When a different type of digit was projected to each eye, the RR and LR groups recalled more digits of both types correctly than did the RL and LL groups (Tables III and IIIA). Further, when groups differing both in handedness and eye-dominance (RR vs. LL) are considered, this trend was very pronounced ($t = 2.59$ for conventional digits, and 2.72 for unconventional ones, $p < 0.02$ in both instances). As a consequence of the speed increase associated with unconventional digits under these conditions, and the slowing of response to conventional ones (see above), differences in speed, unlike those for accuracy, did not depend upon *type* but more upon *origin* of the digits. When each eye is considered separately, information from the left eye is seen to have been reported most rapidly by subjects who were right eye dominant. Though somewhat less marked, similar results were shown for digits from the right (Table IVA). Thus, when a different type of digit was projected to each eye, speed and accuracy of response again depended upon handedness and eye-dominance.

TABLE V

MEAN FREQUENCY WITH WHICH FIRST RESPONSES WERE IDENTIFIED WITH A PARTICULAR EYE WHEN THE SAME TYPE OF DIGIT WAS PROJECTED TO EACH EYE¹

Group				Conventional digits		Unconventional digits	
				Left eye	Right eye	Left eye	Right eye
RR	..	..	..	10.0	0.0	9.6	0.4
RL	..	..	..	9.4	0.6	9.2	0.8
LR	..	..	..	9.4	0.6	9.2	0.8
LL	..	..	..	10.0	0.0	9.2	0.8

¹ Maximum value possible in any category = 10.0.

It should be borne in mind that the first response on any trial was not only slower (overall $\bar{X} = 2.07$ sec.) than the remainder but was almost invariably a digit projected to the left eye (Tables V and VI), and this was independent of handedness or eye-dominance. In contrast, speed and accuracy of reporting subsequent digits was a function of handedness and eye-dominance.

Supplementary analyses showed that the mean percentage of unconventional digits recalled correctly (76.4 per cent.) when an unconventional digit was projected

TABLE VI

MEAN FREQUENCY WITH WHICH FIRST RESPONSES WERE IDENTIFIED WITH A PARTICULAR EYE WHEN A DIFFERENT TYPE OF DIGIT WAS PROJECTED TO EACH EYE¹

Group	Conventional digits		Unconventional digits	
	Left eye	Right eye	Left eye	Right eye
RR	9.2	0.0	9.4	1.4
RL	9.0	0.8	8.8	1.4
LR	9.0	1.6	8.0	1.4
LL	9.6	0.8	9.0	0.6

¹ Maximum value possible in any category = 10.0.

to one eye and a conventional one to the other was significantly better ($p < 0.001$) than when the same type of digit (in this case both unconventional ones) was projected to each eye. In the latter instance, the mean recall was 67.5 per cent. correct. In contrast, the percentage of conventional digits recalled correctly was almost identical under both conditions being 82.3 and 82.5 per cent. respectively. These findings are independent of handedness and eye-dominance differences.

Finally, responses were typically reported as single digits grouped temporally in pairs with information projected to the left eye reported consistently first, and significantly slower than that for the right eye for all groups under both experimental conditions.

DISCUSSION

One major outcome of this experiment was the superiority of right eye-dominant groups, particularly the RR group, in both accuracy of recall and speed of response compared with the left eye-dominant groups, especially the LL group. In recall this superiority was demonstrated unequivocally when the same type of digit was projected to each eye, only with respect to digits projected to the right eye, performance on the left being similar in all experimental groups. However, when a different type of digit was projected to each eye, the weakness in recall of the left eye-dominant subjects, particularly those who were left handed as well, was apparent in both eyes in that the right eye-dominant groups recall both newly learned (unconventional) and overlearned (conventional) digits more accurately. The results for speed are in general agreement with those for recall, and show that speed of response differentiates between the groups even more precisely. Response was slower for information from the left in all groups, but the left eye-dominant groups were slower than right eye-dominant groups under both experimental conditions in response to digits projected to left and right eyes. These results suggest that further experimental study of relations between handedness, eye-dominance, and immediate memory using the present technique might yield patterns of behaviour of use subsequently in neurophysiological study of the relations between temporal lobe function and memory (Penfield, 1958; Milner, 1958).

Speaking generally, these results are in accord with Broadbent's findings (1956, 1957) that the ears can function simultaneously as independent information sources. However, they also suggest an important difference between vision and audition in the ordering of digits recalled correctly.

In agreement with Broadbent's conclusions are the two significant interactions reported here between amount remembered from each eye and handedness and eye-dominance; and between speed of reporting information from each eye, handedness and eye-dominance. The same trend is suggested by the significant difference between the groups in speed of response associated with each eye when a different type of digit was projected to each. In principle, then the eyes seem able to function independently as sources of information.

The need to restrict the generality of Broadbent's conclusion based upon auditory stimuli is seen in the fact that present subjects did not report *all* information projected to one eye first. Rather they almost invariably reported information projected to the left eye first. Responses were also reported singly but grouped temporally in pairs with information from the left the slowest in each pair. At first glance this might suggest that differences attributed above to differences between the eyes as information sources might well be accounted for in terms of an habitual left-right response tendency originating from reading habits. This cannot be so because differences between the eyes in speed and accuracy of response were associated with handedness and eye-dominance, and moreover, the right eye material was recalled more accurately than the left. The capacity of the eyes to function simultaneously and independently therefore seems to depend on eye-dominance and handedness, while serial ordering of responses depends on response mechanisms associated with vision that seem more stably organized and stereotyped than in audition and to reflect both temporal and spatial features of the stimulus field. The possibility of such an habitual response tendency could account for failure of these subjects to give all information from one eye first.

In passing, it is worth recalling the fact that though the same general trends were shown in the case of the overlearned and newly-learned digits, there was one significant exception. This was that newly-learned digits were recalled better in the mixed presentations with a conventional digit projected to one eye and an unconventional (newly-learned) digit projected to the other than when the same type of digit was projected to each eye. This was not the case for conventional, overlearned material, it will be remembered. A possible influence of shifts in attention from one "channel" to the other in recall is suggested here.

One final point. It is common experience in binocular viewing that a person may be dominant in one eye and yet this dominance be subject to momentary fluctuations. Variations in speed and accuracy of recall with eye-dominance suggest that shifting dominance may be one mechanism by which spatially distributed cues come to be organized serially in memory.

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REFERENCES

- BROADBENT, D. E. (1956). Successive responses to simultaneous stimuli. *Quart. J. exp. Psychol.*, **8**, 145-52.
- BROADBENT, D. E. (1957). Immediate memory and simultaneous stimuli. *Quart. J. exp. Psychol.*, **9**, I-II.
- BROADBENT, D. E. (1958). *Perception and Communication*. London.
- FOLEY, P. J. (1956). Evaluation of angular digits and comparisons with a conventional set. *J. appl. Psychol.*, **40**, 178-80.
- LANSDELL, H. (1954). Effect of form on legibility of numbers. *Canad. J. Psychol.*, **8**, 77-9.
- MILNER, Brenda (1958). Psychological effects produced by temporal lobe excision. *Res. Pub. Ass. nerv. ment. Dis.*, **36**, 244-57.
- PENFIELD, W. (1958). Functional localization in temporal and deep Sylvian areas. *Res. Pub. Ass. nerv. ment. Dis.*, **36**, 210-26.

APPARATUS

A COLLIMATOR STABILIZING SYSTEM FOR THE RETINAL IMAGE

BY

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When an observer is fixating a stationary target as closely as possible, continuous involuntary rotations of the eyeball cause proportional displacements of the retinal image. In order to study the functional importance of these movements in the visual process, many methods of maintaining the image in a fixed retinal position have been reported. The limitations of each experimental technique impose restrictions on the type of problem that can be investigated. A method of producing a stabilized image is here described. It is simple, cheap to construct and yet overcomes most of the limitations of the methods referred to below.

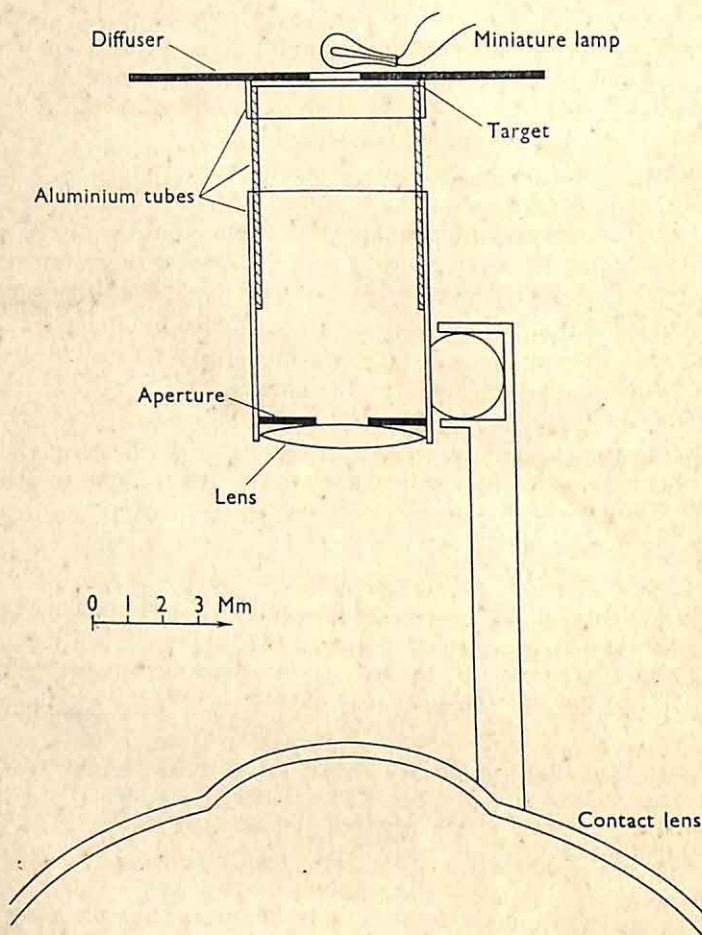
The optical lever methods of stabilization (Ditchburn and Ginsborg, 1952; Riggs *et al.*, 1953; Ditchburn and Fender, 1955; Cornsweet, 1956; Krauskopf 1957; Clowes and Ditchburn, 1959) can only compensate for rotations about two axes simultaneously. The earlier stabilizing systems in which the material forming the targets is directly attached to the eye (Ditchburn and Pritchard, 1956) have severe limitations since the variety of possible forms of target is restricted. The same limitation applies to the systems in which the internal structure of the eye is imaged on the retina (Ten Doesschate, 1954; Ditchburn *et al.*, 1956; Ratliff, 1958). The high power contact lens methods (Mackay, 1957; Pritchard, 1958; Ditchburn and Pritchard, 1960) enable more suitable forms of target to be used but the poor optical quality of the high power Perspex lens affects the quality of the retinal image. A considerable improvement is achieved with the replacement of the Perspex lens by a good quality glass lens (Yarbus, 1957). The required use of anaesthetic and the taping back of the eyelids interfere with the normal state of the subject.

In the method to be described, a high power glass lens of focal length 10 mm. (an achromat, although a simple double convex lens has been found to be equally successful) is cemented inside a thin-walled seamless round aluminium alloy tube. The photographic negative that forms the target to be viewed rests on another thin-walled aluminium alloy tube which is a telescopic fit to the lens support tube. The image producer is brought into the focal plane of the lens by slide adjustment and the two tubes are sealed in this relative position. A third aluminium alloy tube with the same internal diameter as the lens support tube, is attached normal to a thin disc of diffusing material (Kodak leader tape). The cap-shaped object produced secures the target in the focal plane (see Fig. 1) and also carries the illuminating system. The source of light is a miniature surgical bulb and leads from the bulb are connected to a two pin socket taped to the forehead. A 1.5 mm. diameter aperture is located at the surface of the lens. The collimator system is cemented to a 3 mm. diameter nylon ball which fits tightly into a ball-socket joint on a Perspex stalk attached to a contact lens. A continuous variation of the location of the image on the retina is

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possible by the rotation of the ball in the Perspex socket. The test object is changed by unscrewing the top cap, replacing the circular photographic disc by another and rescrewing the cap. The new target is immediately in the focal plane of the high-power lens. The collimator has a total mass of 0.27 gm.

FIGURE 1



The socket taped to the forehead is in series with a 3-volt d.c. supply and a rheostat to enable a continuous variation of the luminance of the field containing the target. The test object may also be viewed against an illuminated screen which is not carried on the eye. A circular disc of coloured filter may be inserted between the target and diffuser for experimentation with monochromatic images.

In common with most stabilizing systems a contact lens is used to follow the movements of the eye. The contact lens worn by the subject must be a tight-fitting one, ground to correct for his ordinary visual defects. The target as seen by the subject is located at infinity by viewing through the collimating lens. It may therefore be seen in focus with a relaxed eye.

The system described above is capable of horizontal, vertical and torsional stabilization over a very large field. An accurate determination of the focal length of the

glass lens is required only when the collimator is first constructed. No subsequent focussing of the system is necessary. The use of anaesthetics and the retraction of the eyelids are not required. The target can be changed during an experiment. The primary components of the system are extremely portable and hence the method can easily be adjusted to other investigations. The system is cheap and simple to construct. In this apparatus, the target is viewed with an effective magnification of about 30, so that a distance of 0.4 mm. at the target represents 2° and a distance of 3μ represents 1 min. of arc in the visual field. The apparatus is thus not suitable for producing small images of very fine definition for some experiments on the central fovea and it does not permit continuous variation of the target as in the bipartite field of a photometer. It is, however, suitable for a wide variety of experiments and for these its simplicity and convenience are very valuable.

Small astigmatic changes in the lens of the eye can produce very small shifts of the retinal image after compensation by an optical system. These astigmatic changes are associated with accommodation changes and occur continuously (Campbell *et al.*, 1959). They disappear, however, when a subject views a target at infinity. This condition is satisfied by the collimator stabilizing system. Gregory (private communication) has suggested that homatropine could be applied to prevent accommodation changes and an aperture stop be added to the system to overcome the related problem of a reduction in image quality due to pupil dilation.

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REFERENCES

- CAMPBELL, F. W., ROBSON, J. G., and WESTHEIMER, G. (1959). Fluctuations of accommodation under steady viewing. *J. Physiol.*, **145**, 579-94.
- CLOWES, M. B., and DITCHBURN, R. W. (1959). An improved apparatus for producing a stabilized retinal image. *Opt. Acta*, **6**, 128-33.
- CORNSWEET, T. N. (1956). Determination of the stimuli for involuntary drifts and saccadic eye movements. *J. Opt. Soc. Amer.*, **46**, 987-93.
- DITCHBURN, R. W., and FENDER, D. H. (1955). The stabilized retinal image. *Opt. Acta*, **2**, 128-33.
- DITCHBURN, R. W., FENDER, D. H., MAYNE, S., and PRITCHARD, R. M. (1956). A stabilized retinal image of the iris. *Proc. Phys. Soc. B*, **69**, 1165-6.
- DITCHBURN, R. W., and GINSBURG, B. L. (1952). Vision with a stabilized retinal image. *Nature*, **170**, 36-7.
- DITCHBURN, R. W., and PRITCHARD, R. M. (1956). Stabilized interference fringes on the retina. *Nature*, **177**, 434.
- DITCHBURN, R. W., and PRITCHARD, R. M. (1960). Binocular vision with two stabilized retinal images. *Quart. J. exp. Psychol.*, **12**, 26-32.
- DOESSCHATE, J. TEN., (1954). A new form of physiological nystagmus. *Ophthalmologica*, **127**, 65-73.
- KRAUSKOPF, J. (1957). Effect of retinal image motion on contrast thresholds for maintained vision. *J. Opt. Soc. Amer.*, **47**, 740-4.
- MACKAY, D. M. (1957). Some further visual phenomena associated with regular patterned stimulation. *Nature*, **180**, 1145-6.
- PRITCHARD, R. M. (1958). Studies of visual perception with a stabilized retinal image. *Ph.D. Thesis, University of Reading*.
- RATLIFF, F. (1958). Stationary retinal image requiring no attachments to the eye. *J. Opt. Soc. Amer.*, **48**, 274-5.
- RIGGS, L. A., RATLIFF, F., CORNSWEET, J. C., and CORNSWEET, T. N. (1953). The disappearance of steadily fixated visual test objects. *J. Opt. Soc. Amer.*, **43**, 495-501.
- YARBUS, A. L. (1957). On the perception of an image fixed relative to the retina. *Biofizika*, **2**, 703-12.

BOOK REVIEWS

Studies in Mathematical Learning Theory. Edited by Robert R. Bush and William K. Estes. London. Oxford University Press (for Stanford University Press). 1960. Pp. viii + 432. 92s.

This book is a collection of twenty papers written by fifteen authors most of whom attended a Summer Institute at Stanford in 1957. It presents an integration and development of a certain kind of learning theory which has received more and more attention during the last ten years. It has the advantage of a fairly consistent use of symbols throughout, though some of this advantage has been lost by there being no glossary or index of symbols. Also a notation which permits the concise reporting of the whole sequence of responses in a learning situation has been adopted and some experimental results have been given in full. Such results could be of great use to those working on theories of learning whether or not they agree with the approach of the authors of these papers.

This approach is briefly summarized below. The authors restrict themselves to considering the sort of learning that occurs when a subject is repeatedly exposed to the same situation, in which he must choose one from a finite number of possible responses; after each choice exactly one of these responses is reinforced. Gradually the response that is reinforced most frequently comes to be made more and more often. The problem is to describe in detail how this occurs, and the answers attempted here depend on the following assumptions:—

- (i) since the situation is always the same the observed change in behaviour must be the result of the responses and reinforcements that occur; thus a record of the responses and reinforcements comprises the whole of the data from such an experiment;
- (ii) knowledge of the first n responses and reinforcements must imply some knowledge of the response which will be made at the $(n + 1)$ th trial. This knowledge is expressible in terms of the probability, for each response, that it will be made on the $(n + 1)$ th trial and these probabilities are functions of the preceding responses and reinforcements only;
- (iii) the probabilities attached to responses will change from one trial to the next; from a single subject only one observation can be obtained at each trial; therefore it is, if not desirable, at any rate almost always necessary to estimate these probabilities from the performance of a group of subjects;
- (iv) the probabilities associated with any trial sum up all the previous history; it is not necessary to know how they arose. Thus the change in probabilities from the n th to the $(n + 1)$ th trial depends only on the response and reinforcement at trial n and the relation between them can be described by an equation containing constant parameters. This is the independence-of-path assumption and is adopted mainly for mathematical convenience;
- (v) as a first approximation the equation in (iv) may be taken to be linear.

Not all the authors make all these assumptions. For instance, Sternberg (Chapter 16) rejects (iv), Bush, Galanter and Luce (Chapter 18) reject (v). Also Suppes (Chapter 19) considers the case of a continuum of responses and adds a "smearing function" which is independent of the response-reinforcement sequence. In general, however, it is fair to say that the book is devoted to investigating the consequences of these assumptions by considering various linear models, some (especially in the first section) derived by psychological argument, others (in the second section) adopted for purely abstract reasons. In the last section, and in certain chapters of the earlier ones, these models are tested against experimental data and compared for adequacy.

Since any theory which implies that learning occurs seems to produce a learning curve very similar to that obtained from averaging the results of a set of subjects, and since there is no sensitive test of goodness of fit of such curves, the criterion of adequacy mainly used in these papers is the extent to which a model can reproduce the "fine-grain structure" of the response sequences. The fine detail of the data is described by the number of errors before the first success, the total number of errors, the number of alternations and other

similar statistics, and the expected values and variances of many of these are calculated for each model. As may be imagined, some very complicated and tedious algebra is required which is not easy to read; moreover, in Chapter 8 a formal exposition of linear models is made by way of set theory, Borel fields and the Kolmogorov probability axioms and this may well daunt non-mathematicians. Is it worth a psychologist's while to master the notation and mathematics needed to understand all this? In any case, should heavy mathematical guns be brought into action until we are certain that they are trained in the right direction?

It seems pertinent to ask what a psychologist requires of a theory of learning, restricting the question to learning in a two-choice situation as the book also does on the whole. Presumably he has in his mind the results of a variety of learning experiments; a suitable theory should organize these results into a sort of map which must agree in broad outline with data already obtained and predict reasonably well the outcome of new experiments; with luck, it may fit in great detail but this is generally too much to ask at first. It is on this point of fitting the data in a general way that the linear models prove inadequate, as is shown in Chapter 14, the clearest and most stimulating chapter in the book. The results and discussion included in it make clear that learning at the beginning of an experiment proceeds much more slowly, and extinction proceeds much more rapidly than the models predict; also, there is a daily recovery effect when trials continue over several days which impels the authors to say that "all the path-independent models must be rejected before they are applied." Thus the fourth of the assumptions stated above is invalid. The authors suggest that the problem of daily recovery should be avoided by change in the experiments; this would mean that the theory was that much less valuable though it might still be worth having. The greatest difficulty, however, lies in the slowness of initial learning, which they attempt to deal with by pretraining. Should initial learning be regarded as something other than learning in order to make the theory fit? Or is there something wrong with the other assumptions?

The reviewer believes that the first assumption, for all its attractive air of scientific detachment, is misleading; it persuades the theorist that experiments which can be recorded in the same way are of the same kind and can be described by essentially the same formulae. There is unfortunately one thing about the experiment that such sequences do not record and that is the amount of information given to the subject. There are three main experiments considered in this book: the two-armed bandit, in which the (human) subject is told that at each trial there is just one correct key; the two-armed bandit, in which the subject is given no information and where, for all he knows, both keys or neither key may be right; and the T-maze experiment, for rats, in which food is put in one arm at each trial. The question is, what sort of problem confronts the subject in these three experiments. In the first the subject may believe it possible to learn which is the better response or, from previous experience with gambling devices, he may not; in the former case he may look for a pattern or in the latter case he must learn which reinforcement schedule with one side reinforced more often; in the latter case he must suppose that reinforcement is random with a probability of one half that each side is correct. In either case, if randomness is assumed, the experiment is equivalent to one in which balls are successively drawn from an urn containing red and white balls, the ball being asked before each trial to guess the colour that will be drawn, and the ball being replaced after he has seen it. Since an attempt by the subject to find a pattern would cause much theoretical complication, this is in fact a more straightforward experiment and one, moreover, for which there is a best strategy: to call whatever colour has so far occurred more often (this seems to have been adopted by five of the subjects whose results are given on page 356). Any deviation from the best strategy must be due to curious human ideas about randomness which could be explored by using an urn in which were equal numbers of red and white balls.

The second experiment, again assuming that the subject believes that there is a random reinforcement schedule, may be represented by taking two urns containing different proportions of red balls. At each trial the subject has to choose the urn from which a ball is to be drawn and he scores a point if the ball is red, so that he must learn to choose the urn in which there is a larger proportion of red balls. The best strategy for this experiment is by no means obvious and the situation is vastly different from that of the first experiment because at each trial he gains information about only one urn. The third experiment is in some ways rather similar to the second in that at each trial the rat receives information only about the arm he visits, but there is an additional complication. If the rat chooses the correct arm and eats the food there it is empty of food when he

leaves it; if he chooses the wrong arm it is likewise empty of food when he leaves it. No one can tell the rat that there is food somewhere in the maze at each trial. The best that even the most reasonable rat could do would be to make decisions in terms of the rate of replenishment. To begin with, this would lead him to avoid the arm that had been visited most recently; later, he should learn that food is replaced more rapidly in one arm than the other. This supposes that the rat recognizes that it is the same maze each time; Galanter and Bush (Chapter 14) in fact found it necessary to give considerable visual cues which presumably supplied this information. If these experiments are discussed in terms of stimulus-response connections they seem to be almost identical; when considered in terms of the problem presented to the subject they are seen to be very different. The flight from anthropomorphism would seem to have gone too far.

Mention has been made of the possibility that a subject might have previous experience of gambling devices; similarly a rat must have had experience of the rate of recurrence of food. Such experience implies a criticism of assumption (iii), that subjects undergoing the same experiment may be regarded as equivalent to each other. Learning need not only take place in an experimental situation; the subject brings to it something which corresponds to prior probabilities or degrees of belief about the conditions he may encounter. The experiment described in Chapter 2 (p. 58) confirms the view that previous experience affects experimental results. Moreover, this experiment implies a criticism of the assumption that the state of a subject at a given trial can be completely described by the probability that he will choose one of the two available responses. There are two reasons, at least, why this probability might have the value one-half; the subject may have no knowledge about the situation, as at the beginning of an experiment, or he may have the firm belief that reinforcement is random and 50 : 50, as should occur at the end of an experiment with such a reinforcement schedule. Clearly, probabilities of response are not in one-one correspondence with states of mind, yet the state of mind may affect the way in which probability changes.

Some uneasiness about the adequacy of these models can be detected throughout the book and several modifications have been proposed though the first two assumptions seem not to have been questioned. Sternberg (Chapters 16 and 17) suggests an interesting perseveration model. A non-linear model, the Beta model, is considered in Chapter 18; this depends on the assumption described as "the independence of irrelevant alternatives," where the term irrelevant is surely misapplied. (It appears to have nothing to do with the theorem on page 147, which is the usual definition of conditional probability.) The appropriateness of this assumption can be examined by reflecting on the classical case of the donkey between two bundles of hay who thought that if one bundle of hay were removed he would cease to be a donkey, and is left as an exercise for the reader; one suspects that it might apply to very restricted situations such as those arising in psycho-physical experiments. Chapters 3 and 6 give clear accounts of some results for Markov chains, though probably no one would expect the one-stage Markov model of the former to describe human learning since it assumes that a subject pays attention to the result of the previous trial alone, and to the extent that a correct response is always repeated. Yet on the last ninety trials the probability of repeating a correct response is 0.86 for one alternative, 0.47 for the other; these are unequal, as well as being different from unity, and initially they must have been approximately equal. On the average, over subjects, there is a bias towards the best strategy, perhaps because some subjects have learnt to adopt this strategy. The experimental data in this chapter and in Chapter 17 may well show subject differences but these have not been looked for. The statement that a two-stage Markov model would fit the data no better is rather surprising in view of the values of chi-squared reported on page 73. The methods of fitting parameters throughout are fairly arbitrary; usually expected values are equated to observed means. Probably nothing better can be done, in the circumstances, but it would be reassuring to know how far the results of Monte Carlo methods might, in consequence, be biased. In Chapter 15, where the goodness of fit of eight models is discussed, no mention is made of the number of parameters fitted, which, for the various models, may be one, two or three.

What has been said above may seem to imply that, in this approach, the real problem of learning has been lost in a maze of computation. Is this a fair judgement? It is perhaps worth recalling what Feller says, in the first Berkeley Symposium on Probability and Statistics, about the rôle of mathematical theories: "As for practical usefulness, it should be borne in mind that for a mathematical theory to be applicable it is by no means necessary that it be able to provide accurate models of observed phenomena. Very often

in applications the constructive rôle of mathematical theories is less important than the economy of thought and experimentation resulting from the ease with which qualitatively reasonable working hypotheses can be eliminated by mathematical arguments. Perhaps even more important is the constant interpretation of observations in the light of theory and of theory in the light of observations; in this way mathematical theory can become an indispensable guide not only to a better understanding, but even to a proper formulation of scientific problems." From this point of view, the papers in this book show a valuable and unusually honest attempt to state certain theories exactly, to test them by experiment and to scrutinize closely many subsidiary problems. The conclusion one reaches from the reading it is that such theories may be eliminated except as an approximation over the "middle range" of learning in rather restricted conditions. The last step, the proper formulation of the problems of learning, has yet to be made, and can only be made with the full realization that the mathematical form suitable for a new field of application is found by trial and error and by intense imaginative effort.

V. R. CANE.

Quantitative Methods in Psychology. By D. Lewis. London. McGraw-Hill. 1960.
xii + 558. 74s.

This book deals chiefly with the mathematics of curve fitting. Material for illustrations is taken mainly from psychological experiments but apart from this there is little of psychology in it. Also, it does not deal directly with statistical methods although it provides a much fuller account of the mathematical basis of certain statistical functions than is usual in elementary texts. Let it be said at the outset that unless the reader has a fair knowledge of mathematics and is accustomed to thinking with mathematical symbols he is unlikely to benefit a great deal from this book. It is true that the first few chapters are written simply and with exemplary pedagogic patience but later discussions require familiarity with such things as determinants, Jacobians, spherical analytical geometry and so on, making the going very heavy at times.

The main thesis of the book is the derivation of equations to describe the functional relationships between experimental variables and the fitting of curves to observational data. Starting with simple linear functions, the author goes on to derive logarithmic and exponential functions and complex functions giving rise to parabolic, hyperbolic and other types of curves. Methods of testing the goodness of fit of such curves to observational data are described with much thoroughness and with fully worked out numerical examples.

There follow two chapters on elementary differential and integral calculus; these form a useful introduction to the nature and uses of calculus but are not meant to be a substitute for ordinary textbooks on the subject. The next three chapters deal with the types of distribution functions commonly encountered in books on statistics, namely, the normal, binomial, Poisson, chi-squared, t and F -ratio distributions. Finally, after an account of testing goodness of fit and trend differences by the use of chi-squared and the F -ratio, there is a lengthy chapter entitled "Applications of Equations." In this, after a certain raiser showing how the findings of Galileo and Newton on the Law of Falling Bodies may be presented in the form of equations, the author gives an account of the mathematical basis of such topics as Fechner's Law, Stevens' Power Law, learning (Thurstone, Hull and others) and Hecht's work on the photoreceptor process, finishing with a mathematical model for two-way communication. At the end of each chapter problems are set; unfortunately no solutions are given.

In his preface the author states that the over-all aim of the book is "to provide the student with enough information to enable him to begin using mathematical procedures in his scientific ventures and to whet his appetite for further knowledge of mathematics." His success in the latter respect cannot be gauged since it depends very largely on the type of reader. As regards the primary aim, it is true that a good deal of information is provided with sufficiently clear instructions to enable a student to apply what he learns to his own experimental data. What one feels is lacking, however, is an explicit indication of the reasons why it may be of value to express the functional relationships between variables in a mathematical form. As the author says (p. 64): "Fitting curves to empirical data is one thing. Rationalizing the outcomes is something else." And again (p. 117): "If the aim of curve fitting is primarily descriptive and analytical, the equation should not be of minor import. If the aim is both descriptive and analytical, the equation should not only fit the data well but should also make sense." The onus is still on the experimenter, therefore, to interpret or make sense of his findings. This he could sometimes do—and often does!—by examining his raw observational data and it remains uncertain how far

exhaustive mathematical analysis would help him. In the final chapter, particularly in the parts dealing with learning, it is evident that assumptions have frequently to be made in order to start off the chain of mathematical reasoning. The validity of such assumptions can be tested only indirectly as a rule.

The book is beautifully printed and contains a great deal of information and instruction for students with a mathematical bent. It should aid them not only in analysing their own experimental findings but also in appraising the published work of others.

E. G. CHAMBERS.

Dimensions of Mind. Edited by Sidney Hook. New York. University Press. 1960. Pp. 281. \$5.00.

This is a symposium of the New York University Institute of Philosophy. It consists of 29 papers divided into three sections: the Mind-Body Problem, the Brain and the Machine, and Concept Formation. The authors include such well-known names as W. Köhler, J. B. Rhine, H. H. Price, N. Wiener (in abstract), S. Toulmin, B. F. Skinner and N. R. Hanson.

The title is rather misleading: the book is not concerned with mental abilities or tests, but with ways of thinking about thought and consciousness in the context of the cybernetic revolution. The papers form a mixed bag, some being almost too slight for inclusion but the best being genuine contributions to classical problems. Most stimulating is Hilary Putnam on "Mind and Machines." He discusses the mind-body problem by analogy with the relation between logical and structural states of a machine. His discussion is sophisticated and stimulating and yet humble, and is perhaps sufficient to justify a rather scrappy book.

R. L. GREGORY.

Art and Illusion. By E. H. Gombrich. London. Phaidon Press. 1960, Pp. 466. 70s.

This book by the Director of the Warburg Institute is based on his A. W. Mellon Lectures delivered in Washington in 1956. Apart from its scholarly contribution to aesthetics, the book will interest many psychologists as a courageous attempt to answer the question of why art has a history by making use of scientific findings in the field of visual perception. Thus the author discusses at some length the operation of "mental sets" and of schematic and projective mechanisms in relation to style and tradition. He has also some interesting things to say about visual illusions. Although there is an undoubted tendency to oversimplify, Professor Gombrich's approach should do much to create interest in the psychology of painting and to provide a much needed bridge between the field of art history and domain of the practising artist.

B. BIRAND-TOGROL.

Space and Sight. By M. von Senden. Translated from the German by Peter Heath, with an Appendix containing Essays by A. H. Riesen, G. J. Warnock and J. Z. Young. Methuen. 1960. Pp. 348. 42s.

In this book we are at last presented with the source material of D. O. Hebb's famous discussion on perceptual learning in infancy. The book was extremely rare in the original German, owing to "enemy" bombing, and has not before been translated into English. Peter Heath has done an excellent job, and Miss Sylvia Schweppe, of the British Museum, who discovered a micro-film copy of the original, and who laboured for ten years to get the book published, deserves our grateful thanks. She very sensibly arranged for three authorities to contribute short essays, which serve to suggest the relevance of the case studies to contemporary thought in philosophy and biology. If this were more general practice, it could well aid communication between specialists.

The translated text consists of von Senden's collected case histories—he never himself investigated a case—of recovery of sight from early and commonly long-standing blindness. The cases date from A.D. 1020 (Arabia) to 1931 (U.S.A.), with the majority in the nineteenth century. They are presented not as individual cases, but rather as illustrating various themes or topics, the book being arranged under headings such as: "The significance of tactual impressions," "Spatial data from hearing," "The attainment of visual space after the operation" and so on. This gives the book a sense of continuity, but has the disadvantage that the background of each case receives little or no description. The theoretical discussion seems to us rather out of date, and decidedly dogmatic on issues which present the utmost difficulty for precise thought. Indeed, as Warnock points out in his useful concluding essay, it is seldom clear what relevance the data presented have upon the issues discussed by the author. In particular, few readers would wish to say

that the blind have no conception of space, or feel that either the evidence cited or the arguments offered clarify our ideas of the space of the blind.

It may be found that Hebb seems to underestimate the significance Senden attaches to emotional disturbances following cataract operations as a factor in the slow development of useful sight. One of the striking features of the cases is severe emotional disturbance, and antipathy to lessons in seeing:—"How comes it that I now find myself less happy than before? Everything that I see causes me a disagreeable emotion. Oh, I was much more at ease in my blindness!" cries a girl described by Mesmer, and Beer makes the comment: "Might not . . . a sort of injured pride contribute something to this transformation, in that they now suddenly find themselves so far behind other people of their age, even in the most trivial matters of knowledge?"

The accounts of the patient's difficulties are quite fascinating, and will interest anyone with an eye for human drama let alone professional psychologists, but the cases were examined with—as it now seems to us, primed with questions—an appalling lack of imagination in devising ways of overcoming the difficulties in communicating with one from the valley of the blind. Even today, on the rare occasions when these cases occur, no special effort is made to examine them in the hospital. The reviewer has had the privilege of examining the most recent case—though not before some weeks after the operation—and indeed the difficulties are very great, but surely some tests could have been conducted; and where attempts were made, as by Latta, they are not discussed by Senden.

This book will no doubt become the Old Testament to Hebb's *Organization of Behaviour*. Critics might well wonder whether Hebb would not have done better to have observed whether babies "feel" round contours with their eyes, rather than rely on accounts of past blind people who must be very used to feeling with their fingers. Hebb argues, in effect, that in these cases we have examples of living fossils, which we can examine to discover primitive, infant, perceptual ability and development. But are they not rather adapted to their curtailed environment, and then forced to live in a world made alien by their blindness? Have we reason to believe that the visual system will remain dormant while the child learns about the world through his other senses? That his visual perception will then develop as though he had not grown up? Can we really hope to hear from the mouths of adults the experiences of babes and sucklings? Whatever the answer, these cases are of remarkable interest and certainly serve to stimulate observation and experiment on problems until recently the preserve of the philosopher.

R. L. GREGORY.

Conflict, Arousal and Curiosity. By D. E. Berlyne. London. McGraw-Hill. 1960.
Pp. xii + 350. 58s.

The explanation of apparently autonomous exploratory or manipulatory activities has for several years been a challenge to orthodox drive theory. These activities ("ludic behaviour") do not seem to arise from detectable physiological or biochemical needs and are without obvious survival value. During the past decade Professor Berlyne's work has become familiar to all those interested in the problem, and his new book represents an extremely valuable review and an attempt to formulate a single comprehensive theory applicable to animal and man.

Berlyne has analysed the circumstances (e.g. novelty, uncertainty, conflict) in which ludic behaviour is specially found, and he suggests that these conditions have a common property, viz. high "arousal potential," which tends to raise the activity of the brainstem reticular formation above the minimal resting or "tonus" level. In the waking animal, the tonus level of reticular activity is possible only when the influx of arousal potential from the environment is at a particular optimum level. A departure in either direction from this optimum leads to heightened arousal which is aversive and drive-inducing, and which may be associated with excitement or with boredom. (According to Berlyne, boredom is a special case which develops when an unduly low influx of arousal potential is insufficient to maintain cortical inhibition of reticular activity).

Exploration of novel or conflict-inducing stimuli with high arousal potential leads to familiarization and habituation. This serves to reduce the arousal potential, and reticular activity can revert to its tonus level. The latter process is drive-reducing and reinforces the preceding exploratory activity, which thus becomes conditioned to all novel stimuli. However, moderate increases in arousal of brief duration are commonly sought by animals already "optimally" aroused. In these cases the reinforcement is said to be provided by the fall in arousal that follows. An example of this "arousal jag" is the infant's delight

in being tossed into the air . . . and caught again. Berlyne explains, perhaps not convincingly, that although heightened arousal is drive inducing and aversive (p. 194), its initiation need not be (p. 196).

Thus the scheme is flexible. Stimuli with high arousal potential may cause an increase, or a decrease, in arousal. And, according to circumstances, an increase in arousal may be associated with reward or with punishment. Or neither. Or both. Other uncontrollable variables include change in the arousal potential of stimuli during their exposure, and the creeping up or down of tonus level according to long-term environmental influences. A few of these instabilities (e.g. the last-mentioned) may be logically unnecessary but the others seem indispensable and must preclude a more rigorous formulation of the theory. More predictive value would have been retained if it were possible to derive some independent, physiological measures of arousal. But here again we find that EEG α -blocking (and related indices) sometimes reflect arousal, but at other times must be interpreted as indicating inhibition of the reticular formation by the cortex.

Thus there can be few instances of ludic behaviour with which the model could be shown incompatible and Berlyne takes the opportunity to extend it to the field of aesthetics and humour. Arousal and the arousal jag are proffered as the universal explanation for mankind's interests in these fields, but it is not made satisfactorily clear why these sometimes lead to laughter (humorous) and sometimes to sighs (aesthetic). And since arousal potential from whatever source is additive and interchangeable, the reader should be offered some clarification of the effects (or absence of effects) on artistic taste of a mildly disturbing taxi ride to the art gallery.

It may be a pity that Berlyne apparently did not consider interpreting his data in terms of behaviour theories based on serial stimulus tropisms (e.g. Watson, 1959; Deutsch, 1960) which, unlike classical S-R theories, seem eminently suited for this sort of problem. But despite any deficiency of his avowedly tentative central thesis, Berlyne's treatment of tangentially related material (e.g. thinking, sensory preconditioning, VTE, felicity in art and wit) abounds with provocative suggestions which make this book well worth the attention of the advanced student, and essential reading for the learning theorist.

There are 30 pages of references and approximately 10 per cent. of these are Russian, with many others from French and German sources.

L. J. HERBERG.

Professional Satisfaction among Swedish Bank Employees. By Uno Remitz. Copenhagen. Munksgaard. 1960. Pp. 422. 86 kr.

This book reports a job satisfaction survey of 1,241 people in Swedish banks during 1956-7. The main conclusions are that job satisfaction increases with status, salary and age, decreases with years of formal education and does not vary with sex. There were also very large differences between the 11 banks studied. There are several unusual features in this study. For example, job satisfaction was measured by a novel method of scaling. There are eight sub-scales of 10 questions each; no attempt was made to find how closely these scales correlated with one another, instead subjects were asked to rate the importance of each scale "in assessing the amount of my professional satisfaction." The total score for each individual is obtained by multiplying his score on each scale by his weighting of the importance of the scale. Precisely the same is done *within* each scale, where again no item analysis was done. The average satisfaction scores are given for different groups of subjects in 161 tables, each showing the results of large numbers of *t*-tests. Instead of the usual methods of multivariate analysis, the author fits regression equations, using a method devised by Törnqvist. For example, $\log(\text{satisfaction}) = 0.477 - 0.106 \log(\text{years of education}) + 0.230 \log(\text{salary in 1955}) + 0.043(\text{formal position})$, etc., etc. These equations are used to show the independent weights to be ascribed to different factors, as well as the total variance which can be accounted for by the variables studied (22.6 per cent.). Personality variables such as neuroticism, intelligence, introversion-extraversion and mental activity-passivity, were assessed by handwriting analysis. The scoring is based on 77 ratings of such variables as "degree of pressure" and "forms of binding" (100 alternatives listed here); these are converted to personality scores by methods given in recent German publications cited. However, nearly all the relationships between traits thus measured and job satisfaction seem to be non-significant.

This is an interesting, original and substantial contribution to the job satisfaction literature, the analysis in particular being very thorough. The presentation however leaves much to be desired: the book is stuffed with tables and formulae, and it is no mean task discovering what was discovered.

MICHAEL ARGYLE.

Les Grandes Activités du Lobe Occipital. Edited by Th. Alajouanine. *Actualités Neurophysiologique. Semaine Neurophysiologique de la Salpêtrière.* Paris. Masson et Cie. 1960. Pp. viii + 361.

This is a companion volume to the symposium on the temporal lobes edited by Dr. Alajouanine five years ago. Like its predecessor, this book is mainly concerned with the manifestations of localized cerebral disease, though some account is given of recent work on the anatomy and physiology of the visual system. It may please psychologists to discover a short though eminently sane chapter on visual sensation from the venerable hand of Professor Piéron. In general, however, the main appeal of this rather unequal symposium is to neurologists and others interested in the analysis of central visual disorder.

O. L. ZANGWILL.

Somatosensory Changes after Penetrating Brain Wounds in Man. By Josephine Semmes, Sidney Weinstein, Lila Ghent and Hans-Lukas Teuber. Commonwealth Fund. Harvard University Press. 1960. Pp. xiii + 91. \$4.00.

Visual Field Defects after Penetrating Missile Wounds of the Brain. By Hans-Lukas Teuber, William S. Balstersby and Morris B. Bender. Commonwealth Fund. Harvard University Press. 1960. Pp. xi + 143. \$4.75.

These two impressive monographs from the Psychophysiological Laboratory at Bellevue bring together some important observations on sensory deficits in cases of penetrating brain injury. In the first, a most careful analysis is given of changes in discriminative sensitivity in the two hands produced by unilateral cerebral lesions. Contrary to expectation, the site of lesion giving rise to maximal defect in the contralateral hand was found to be not precisely identical in the two hemispheres and the pattern of "cortical sensory loss" was also somewhat different on the two sides of the body. In the light of their findings, the authors conclude that the cortical representation of sensory hand-function is not precisely the same for the two hemispheres, being more highly "concentrated" in the left sensory-motor region in the case of the right hand. This finding may perhaps be related to other recent evidence of asymmetry in cerebral hemisphere function.

The second monograph deals with the form and distribution of visual field defects in relation to the layout of the visual pathways and the retino-cortical projection system. Although many of the findings accord well with earlier observations, e.g. those of Holmes and Spalding, some discrepancies are noted. In particular, a lack of precise congruence in homonymous field defects was surprisingly common. The authors have some interesting things to say about accommodation to field defects and the vexed question of macular sparing. They also lay stress on the incidence of appreciable functional defect in perimetrically intact portions of the visual field—although the significance of this for disorders of perception (agnosia) is perhaps overstressed. This monograph should be read with care by everyone interested in the cerebral basis of vision.

It is to the credit of Dr. Lukas Teuber and his co-workers that their studies were carried out with an attention to method all too rare in the analysis of neurological material. While in the praiseworthy tradition of Head and Holmes, their work exhibits a technical sophistication much more characteristic of the laboratory than the clinic. These two monographs should do much to convince experimental psychologists that the study of brain injury is not only fascinating but reputable.

O. L. ZANGWILL.

The Role of Speech in the Regulation of Normal and Abnormal Behaviour. By A. R. Luria. Edited by J. Tizard. Oxford. Pergamon Press. 1961. Pp. ix + 100. 50s.

This short book consists of three lectures given in London in 1958. It describes recent research in the Soviet Union on the development of verbal control over other motor processes. This is a field which has received relatively little attention in the West, where we have been concerned more with speech as communication or with the acquisition of verbal skills and word recognition as such.

Starting from a broadly Pavlovian concept of behaviour as sequences of stimulus-initiated responses, Luria develops Pavlov's conditioning model and culminates in the "ling system," which profoundly modifies the conditioning model and culminates in the achievement by the adult of self-regulating "voluntary" control both of attention and of behaviour. Far from being endogenous, these are dependent in the child on learning through social interaction with adults, whose words select and emphasise particular aspects of the environment. Interesting comparative studies of children from different age-groups

required to perform simple stimulus-response tasks (pressing or refraining from pressing a rubber bulb in response to positive or negative signals), reveal three stages in the development of verbal control: in the first the "impelling" excitatory effect of any verbal instruction can only initiate or increase the child's response; in the second it acquires an inhibitory effect, and finally it takes on a selective and directive role, controlling responses by the significance of the stimulus within the verbal framework rather than by its intensity. The ability to inhibit a response is shown to originate from an incompatible excitatory response, itself evoked by a second signal. This leads to a new application of the concept of feedback: the response-produced stimuli may either evoke more of the same responses (positive feedback), as in very young children, or may terminate them by evoking a new, incompatible response, whose overt performance may later gradually come to be omitted. Thus Luria describes a child pressing a rubber bulb in response to a light signal and then moving his hand to his knee on a verbal instruction. This second movement is gradually decreased and then dropped altogether, but the child has learned to inhibit any extra squeezes of the rubber bulb, so achieving the "beginnings of the organization of volitional movement."

As he grows older, the child learns to produce the verbal signals himself, and so to excite, inhibit or direct his own responses. By stressing this use of speech by the child to direct his own activity, Luria throws light on a possible function of the "egocentric" speech, analysed in its structural aspects by Piaget.

In the last lecture, Luria suggests some interesting applications of these ideas to pathological cases. He considers the inter-relations of motor processes with the regulating speech systems, their breakdown and possible compensatory activity. Severely subnormal children may never get beyond the "impelling" stage of verbal control, while less affected children can be trained to use speech to compensate for excitatory or inhibitory imbalance in motor reactions.

These lectures were not intended to include comparisons with Western research, although in some instances these might prove illuminating. Could the excitatory and inhibitory stages of feedback control, for example, be related to the defect in stuttering, shown by Cherry to decrease when auditory feedback is cut out or replaced by other stimuli? Or the findings on the later development of inhibitory responses, to studies of the use of positive and negative information in more complex processes such as concept formation in adults?

The experiments Luria describes are ingenious and interesting, although the absence of details and accounts of controls from this presentation may be a little troublesome to anyone unfamiliar with the research. The theoretical framework seems sometimes more classificatory than explanatory, inferring for example "changes in the degree of concentration and mobility of the processes in the motor analyser" when the overt reactions acquire such characteristics. The model of a simple voluntary act, still in terms of stimuli evoking responses, but with self-generated speech stimuli increasing the range and complexity of discriminations and responses available, seems only to take the problem of voluntary control one stage further back—to the choice of verbal signals. One might also question the assumption that such behaviour has the same "reflex conditioning" basis as the dog salivating to a bell, since Luria himself points out so many important differences—the change in the "law of force of stimuli," in the speed and reversibility of conditioning and extinction, and in their dependence on reinforcement. There is some slurring of the distinctions between words as "stimuli," "signals," "responses" or "links." However the greatly increased range and complexity of behaviour which Luria has brought within his broadened framework has opened a promising field of research. Different initial hypotheses suggest different types of investigation, and the approach of Luria and his colleagues has proved fruitful in pointing out many empirical features of verbal development which will stimulate thought elsewhere.

ANNE TREISMAN.

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Part 4

A FURTHER INVESTIGATION OF APPARENT SIZE AND RETINAL SIZE AS DETERMINANTS OF THE FIGURAL AFTER-EFFECT

BY

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A figure which is more distant from the eye than another but subtends the same visual angle is usually seen as the larger. This phenomenon is called perceptual size constancy. Experiments have been reported which show that sometimes the apparent and sometimes the retinal sizes of the stimulus figures determine the direction of the figural after-effect. These experiments are reviewed and two further experiments are reported. In the first experiment two types of stimulus figure (discs and annuli) which subtended a visual angle of $1^{\circ} 36'$ resulted in figural after-effects, the direction of which would have been predicted by the retinal sizes of the figures. In the second experiment larger figures were used which subtended a visual angle of about 4° and which were the same as those used by Sutherland (1954). In this experiment the after-effects were generally in a direction which would have been expected if apparent size determined the direction of the after-effect. In the interpretation of the results attention is drawn to the "Central Tendency Effect" which has been observed in size judgements of stimulus figures of varying size. Two hypotheses are suggested; one is that the true figural after-effect has been obscured by the "Central Tendency Effect" in successive judgements, and the other is that time error in successive judgements of size is essentially a figural after-effect. Some supportive evidence for the latter hypothesis is reported. It is concluded that apparent size is not a critical determinant of the figural after-effect.

INTRODUCTION

The reported size of an after-image varies as a near linear function of the distance between the eye and the surface on which the image is projected. This near linear relationship between the apparent size of the after-image and the physical distance of the surface is referred to as Emmert's Law. Prentice (1947) first raised the question concerning the relationship between the magnitude of the figural after-effect (FAE) and the distance between the eye and the surface on which the test figure (TF) is presented. On the assumption that the processes underlying the FAE are cortical in origin, Prentice attempted to establish whether the FAE was a function of the apparent size or the angular (retinal) size of the TF. If angular size determines the magnitude of the after-effect then Emmert's Law would apply to this phenomenon as well as to the after-image.

Prentice (1947) used a large rectangle as inspection figure (IF) and two small squares aligned horizontally as TF. The IF was located slightly above the right square of the TF so that the FAE was indicated by a report of the right square lower than the left. The IF remained fixed at 2 m. while TF was placed at 2 and 6 m. from the observer. It was found that although a significant downward displacement of the TF occurred at both distances there was no difference between the magnitude

of displacement for the two conditions. Prentice (1947) confirmed this result in a second experiment in which TF was placed at distances of 3, 5 and 7 m. It was concluded that apparent rather than angular size determined the magnitude of the figural after-effect.

The results of the investigation by Prentice are curious and difficult to interpret in terms of the retinal image of the IF. At 6 m. the retinal projection of the TF was three times smaller in its linear dimensions than at 2 m. Further, the retinal distance between the adjacent contours of IF and TF would have increased. Since the separation between contours is an important determinant of the magnitude of apparent displacement of TF it would be expected that the downward displacement would vary with distance of TF. Since this displacement did not change with distance it was reasonable to conclude that apparent size rather than angular size determined the magnitude of displacement.

There is one further aspect of this experiment which has not been examined so far but which casts considerable doubt upon the conclusions which were reached. In order to maintain the same geometrical relationship between the retinal representations of IF and TF as the distance of TF increased it would also have been necessary to increase the distance between the point of fixation and the contour of TF (see footnote). As far as can be gathered from the account of the experiment, this distance remained constant and the distance from the eye alone was varied. In terms of the retinal representations of the two figures the TF would not have fallen directly below IF but below and to one side of it so that in terms of available data no outcome could be predicted. If, then, it is concluded that the apparent size of the TF determined the displacement from IF it must also be concluded that the apparent distance between the TF and the fixation point rather than the angular distance between them was the effective variable. This is a curious result since not only the apparent sizes of the near and far figures determined the FAE but also their apparent positions in space.

In a second paper Prentice (1950) has reported an experiment based upon an unreported study by Thouless. Thouless pitted apparent size against retinal size so that if FAE was determined by apparent size the after-effect would be in a direction opposite to that to be expected if it were determined by retinal size. Prentice arranged the stimulus figures so that the circular IF viewed at 12 ft. was apparently larger but retinally smaller than the TF viewed at 6 ft. If angular size determined the FAE it was expected that an expansion of TF would have occurred and that a shrinkage would have occurred if apparent size was the principal determinant. Since a significantly greater frequency (69 per cent.) of expansions than of shrinkages (21 per cent.) occurred it was concluded that angular size was the more important determinant of the FAE.

Using an experimental arrangement in which angular size would be expected to produce a shrinkage and apparent size an expansion, Hochberg and Bitterman (1951) quantified the results obtained by Prentice (1950), using the method of adjustment.

Sutherland (1954) argued that in the experiments outlined above apparent size and angular size had been regarded as mutually exclusive and the experimental designs had been such that for the most part angular size would be the most effective

When the sizes and distances of the two stimulus figures have the ratio 2.5:1 it can be shown trigonometrically that the distances from the fixation point to the contours of the figures must have the ratio 2.5:1 in order that the retinal projections of the figures coincide. Sutherland (1954) reports these distances as having a ratio 3:1. In private correspondence, however, he has stated that one of the values given in his paper was incorrect, and that the values he in fact used were such that the distances between the fixation point and the contours of the near and far figures had the ratio 2.5:1.

determinant. Since Prentice (1947) had adduced evidence favouring apparent size as a determinant, and Prentice (1950) and Hochberg and Bitterman (1951) had found angular size to be the principal determinant, both could be effective depending upon the experimental conditions employed.

Sutherland used an IF and a TF of the same angular size but of different apparent sizes. The diameters of the circular figures were 4 and 10 in. and the viewing distances were 57.6 and 144 in. respectively. That is, the sizes and distances of the near and far figures were in the ratio 1:2.5. With the small near circle as IF and the large far circle as TF an expansion of the latter was expected if apparent size was a determinant. When the large far circle was used as IF and the small near circle as TF, a shrinkage of TF was expected in terms of apparent size. The results indicated that apparent size determined the direction of the FAE.

McEwen (1959) using the method of adjustment attempted to quantify the results obtained by Sutherland (1954). The TF consisted of two illuminated discs 150 cm. from the eye. These were set at 30 mm. diameter and could be adjusted in size by means of an iris diaphragm. A near IF at 75 cm. had an outer diameter of 15 mm. and a far IF placed 300 cm. from the eye had an inner diameter of 60 mm. Since the small near IF was apparently smaller than the TF it was expected that an expansion of TF would occur following fixation if apparent size was effective. In this condition McEwen found that a shrinkage occurred in the case of six out of 11 observers and the mean change was a slight but insignificant shrinkage. Thus Sutherland's results for this condition were not confirmed.

A basic error has occurred in the experiment by McEwen which renders the results quite inconclusive. Whereas the near IF had an outer diameter (15 mm.) of the same angular size as TF (30 mm.) there was also an inner contour to the annulus. The annuli were constructed by removing paint from a glass surface. These annuli were 2 mm. wide so that the inner diameter was 11 mm. Since this inner contour was less in angular size than TF it would be expected that fixation would result in an increase in the perceived size of the latter. Briefly, then, there were two contours in the small near IF; an outer contour equal in angular size to TF and an inner one which would be expected to produce an expansion. In the case of the large far IF the inner diameter of the annulus was equal in angular size to TF but the outer diameter of the annulus was 4 mm. greater. Thus two factors, a difference in angular size of the outer contour of IF and a difference in apparent size, could have resulted in the shrinkage.

Köhler and Wallach (1944), Walthall (1946) and Japanese investigators (Sagara and Oyama, 1957) have reported that when IF and TF are of the same retinal size a shrinkage occurs. Thus that condition in which IF and TF are of the same angular size but IF is apparently larger (i.e. physically larger but at a greater distance) than TF does not throw any light on whether or not apparent size determines the FAE. Under this condition both apparent and angular size would be expected to result in a shrinkage of TF. The critical condition for investigating the effect of a difference in apparent size on the figural after-effect when the angular sizes of two figures are the same is that in which IF is near and TF far. In this case angular size would be expected to result in a shrinkage whereas the difference in apparent size would tend to produce an expansion.

In the experiments to be reported here all possible distance relationships between IF and TF have been used including those in which both figures were either near or far. Although the condition in which IF is far and TF is near does not, as already shown, distinguish between the effects of apparent and retinal size, it has been included in order to replicate Sutherland's (1954) experiment. It was also thought

important to exhaust the possible number of distance relationships between the two figures with retinal size constant.

EXPERIMENT I

The results obtained by Sutherland (1954) for the critical condition in which an IF of the same angular size as the TF was placed nearer to the eye have not been confirmed by the data reported by McEwen (1959). In any case, the defects in the stimulus figures used in the latter experiment have rendered the results equivocal. Neither experiment has included the control conditions in which both IF and TF are placed at the near or the far positions. These conditions are necessary in order to separate the effects due to distance from those due to differences in apparent and angular size.

The purpose of the present experiment was to study the direction of the change in the perceived size of a TF following fixation of an IF of the same angular size but of both larger and smaller apparent sizes. The two control conditions in which both IF and TF were presented near and far have been included. Earlier unreported data obtained by the writers suggested that the apparent shrinkage of a figure following fixation of a similar figure occurred more frequently with solid than with outline figures. Köhler and Wallach (1944) have also shown that solid figures result in a greater after-effect than outline figures. For this reason both discs and annuli were used as stimulus figures in the present experiment.

Apparatus. The discs and annuli were drawn with black ink on white art paper, the discs being essentially the annuli "filled in." The small figures were $1\frac{2}{16}$ in. in diameter and the large figures twice this size, $3\frac{1}{8}$ in. in diameter. In the case of the annuli the outlines were $\frac{1}{8}$ and $\frac{1}{4}$ in. wide for the small and large figures respectively. The fixation points for the small and large figures were $\frac{1}{16}$ and $\frac{1}{8}$ in. in diameter and the horizontal distance between the centre of the fixation point and the outer contour of the figure $1\frac{7}{32}$ and $2\frac{7}{16}$ in. respectively.

The papers on which the small figures were drawn were mounted on boards 9 in. square and the papers bearing the large figures on boards 18 in. square. The fixation points coincided with the centres of the boards. Each inspection pattern consisted of a single figure (IF) to the right of the fixation point and each test pattern of two identical figures placed symmetrically on either side of the fixation point. The figure on the right was the TF and that on the left a comparison figure (CF). Large and small figures were drawn for both discs and annuli.

The boards on which the stimulus figures were mounted were in turn mounted on wooden rods which could be fitted into holes drilled in a plank so that the fixation points for the small near figures and the large far figures were 72 and 144 in. from the eye. The visual angle subtended at the eye by the figures was about $1^{\circ} 36'$. The boards were vertical and in the fronto-parallel plane. A head rest was mounted at one end of the plank so that the position of the eye remained fixed. The plank was clamped to two tables so that the fixation points were at the approximate height of the eye. All observations were made under artificial lighting with a lamp arranged so that the brightness of the near figures was approximately the same as that of the far figures.

Experimental conditions and theoretical expectancies

There were eight experimental conditions. These were:

- (1) Small annulus IF at 6 ft. and large annulus TF at 12 ft. ($I_n T_f$) so that the angular sizes of the figures were the same but the apparent size of TF greater than that of IF. If angular size determines the figural after-effect it would be expected that following fixation TF would appear smaller than CF. The difference in apparent size would be expected to result in an apparent expansion of TF.
- (2) As in (1) using discs.
- (3) Large IF at 12 ft. and small TF at 6 ft. ($I_f T_n$) so that angular sizes were the same but the apparent size of IF was greater than that of TF. It was expected that

both angular and apparent size relations between IF and TF would lead to an apparent shrinkage of TF.

- (4) As in (3) using discs.
- (5) Large IF and large TF both at 12 ft. ($I_l T_l$) so that the angular and the apparent sizes of the two figures were the same. It was expected that TF would appear smaller if angular size determines the after-effect.
- (6) As in (5) using discs.
- (7) Small IF and small TF both at 6 ft. ($I_n T_n$) so that angular and apparent sizes were the same for the two figures. It was expected that in terms of angular sizes TF would appear smaller following fixation of IF.
- (8) As in (7) using discs.

Subjects. There were 80 male and female subjects, 10 in each group. Subjects included undergraduates, graduate students, members of the academic staff and clerical assistants.

Procedure. Under all conditions the subject was required to compare TF with CF before fixation of IF (fore-test), and immediately following fixation (after-test). The change in the compared size of TF between fore-test and after-test has been taken as the FAE.

Monocular vision using the right eye only was used throughout the experiment while the left eye was covered with an eye patch. In order to position the eye correctly the subject, after placing his chin in the rest, was instructed to move his head backwards and forwards until the near 9 in. board was exactly coincident with the far 18 in. board. In this position the retinal projections of the near and far stimulus figures were exactly coincident. With his head held in position the subject compared the relative sizes of TF and CF after which the fixation point of the IF was steadily fixated for 2 min. Following fixation the test pattern was again placed in position and a second comparison of the relative sizes of TF and CF made.

The following instructions were given before the first comparison was made: Look at the dot between the two circles. Do the circles appear the same or different in size and, if different, how are they different? The nature of this experiment is such that it is absolutely essential that you hold your head perfectly steady in that position throughout. When I tell you I want you to gaze at the dot to the left of the circle for the 2 min. When the 2 min. are up I will warn you and then take the figure away and replace it with another. While I am doing this do not shut your eyes or shift your gaze. When I place the second figure in position your gaze will fall on the dot between the circles. Your task is to remain looking at the dot and to compare the relative sizes of the two circles on either side of it. I want you to tell me whether they look the same or different in size and I want you also to give me your first impressions with as little delay as possible. Is that clear?

Results. Following fixation of IF for 2 min. the judgements of the size of TF relative to CF could be larger than (l), equal to (e) or smaller than (s) the judgements before fixation. In those cases in which the subject judged TF larger or smaller than CF prior to fixation and gave a similar judgement after fixation, he was asked if there was any marked difference between the two judgements. A difference was recorded only when the subject was quite convinced that there was such a difference. The frequency of occurrence of the three categories of judgement for the 8 conditions is shown in Table I. If the experimental conditions used had failed to produce after-effect, it would be expected that judgements would be equally distributed among the three categories. In order to establish that a significant departure from this pattern of distribution occurred the total obtained frequencies for the two types of stimulus figure has been compared with the frequencies expected from an equal distribution among the categories using the chi-squared method. This test has shown that the obtained total frequencies are significant beyond the 0.01 level. From Table I it can be seen that for all conditions the greater number of entries fall into either the s or e categories with only six of the 80 (7.5 per cent.) falling into

the l category. The only condition in which this trend in distribution does not occur is $I_f T_f$ for annulus figures. That is, following fixation there was a very strong tendency for a shrinkage or no change to occur. This happened with both the annuli and the discs.

The frequencies in Table I have been tested using the exact probability treatment developed by Myers (1958) since with this method the outcomes are not affected, as in the chi-squared technique, by small or zero frequencies in the cells. The exact probability analysis has shown that there is no significant difference between any

TABLE I

FREQUENCY OF DIFFERENCES BETWEEN JUDGEMENTS OF TF AND CF BEFORE AND AFTER FIXATION FOR ANNULI (A) AND DISCS (D). THE ENTRIES REFER TO TF JUDGED SMALLER THAN (s), EQUAL TO (e) AND LARGER THAN (l) CF

Conditions		Experiment I									
		IF near, TF far ($I_n T_f$)		IF far, TF near ($I_f T_n$)		IF near, TF near ($I_n T_n$)		IF far, TF far ($I_f T_f$)		Total	
		A	D	A	D	A	D	A	D	A	D
Figures											
<i>s</i>	4	10	5	8	7	8	3	7	19	33	
<i>e</i>	5	0	4	2	3	2	3	7	15	7	
<i>l</i>	1	0	1	0	0	0	4	0	6	0	
Total	10	10	10	10	10	10	10	10	40	40	

Experiment II											
<i>s</i>	0	4	5	6	5	4	2	4	12	18	
<i>e</i>	4	5	4	2	1	4	2	1	11	12	
<i>l</i>	8	3	3	4	6	4	8	7	25	18	
Total	12	12	12	12	12	12	12	12	48	48	

pairing of the four conditions of distance relationship. These analyses have been carried out separately for annuli and discs. In the case of the differences in frequencies between annuli and discs for equivalent distance conditions, the sole significant difference is that between the $I_n T_f$ conditions. This is the only significant difference among the 16 independent comparisons made. The exact probabilities for the 16 comparisons are shown in Table II.

The tendency towards either no change in judgement between fore-test and after-test or an apparent shrinkage of TF relative to CF is in agreement with what would be expected for retinal size as the principal determinant of the FAE. There is no suggestion in this data that apparent size determines the after-effect since for both annuli and discs in the $I_n T_f$ condition only one of the 20 (5 per cent.) FAEs is in the l category.

It should be noted that the magnitude of the FAE is determined by the separation between the contours of IF and TF and that the displacement is greatest when the contours are separated by a certain critical distance. This is the "distance paradox" first mentioned by Köhler and Wallach (1944) and confirmed by Pollack (1958).

When the separation between contours is zero (i.e. the contours of IF and TF are coincident) the FAE is very slight so that it is conceivable that the magnitude of change in the TF would be below threshold for many observers. This would partially explain the occurrence of e entires in the distributions.

Whereas in Sutherland's experiment binocular viewing was used throughout, in the present experiment monocular viewing was used. Although in terms of ocular geometry these two methods of viewing should give the same results, eight extra subjects were tested with the annulus figures under the critical I_nT_f condition using both binocular and monocular vision. Half the group viewed first monocularly and the other half viewed first binocularly. The frequencies of s , e and l entries did not

TABLE II
EXACT PROBABILITIES BETWEEN FREQUENCIES FOR EXPERIMENTS I AND II

Experiment I							
Within groups					Between groups		
Figures	Discs		Annuli		Figures	Discs and Annuli	
Conditions	p	Signifi- cance	p	Signifi- cance	Condi- tions	p	Signifi- cance
I_nT_f and I_nT_n	0.471	NS	0.372	NS	I_nT_f	0.010	S
I_nT_f and I_fT_n	0.473	NS	1.000	NS	I_nT_n	1.000	NS
I_nT_f and I_fT_f	0.211	NS	0.396	NS	I_fT_n	0.354	NS
I_nT_n and I_fT_n	1.000	NS	0.653	NS	I_fT_f	0.076	NS
I_nT_n and I_fT_f	1.000	NS	0.073	NS			
I_fT_n and I_fT_f	1.000	NS	0.391	NS			

Experiment II							
I_nT_f and I_nT_n	1.000	NS	0.021	S	I_nT_f	0.023	S
I_nT_f and I_fT_n	0.220	NS	0.012	S	I_nT_n	0.219	NS
I_nT_f and I_fT_f	0.067	NS	0.194	NS	I_fT_n	0.843	NS
I_nT_n and I_fT_n	0.467	NS	0.170	NS	I_fT_f	1.000	NS
I_nT_n and I_fT_f	0.169	NS	0.025	S			
I_fT_n and I_fT_f	0.263	NS	0.036	S			

differ significantly and the distribution of frequencies for the two conditions of viewing was essentially the same as in the top half of Table I.

In summary, the results from this experiment indicate that differences in apparent size between IF and TF do not determine the direction of apparent change in size following fixation and that, for the most part, the changes are in a direction that would be expected from the retinal dimensions of the stimulus figures.

EXPERIMENT II

The results from Experiment I are quite different from those reported by Sutherland (1954) who found that when IF was nearer than TF but both subtended the same visual angle (I_nT_f) there was a tendency for expansions to occur. In Experiment I the angle subtended at the eye by the total horizontal distance from the outer contour of TF to the outer contour of CF was $4^\circ 24'$ whereas in Sutherland's experiment this angle was $9^\circ 22'$. It seemed possible that such a large difference between

angular dimensions of the two sets of stimulus figures could contribute to the discrepancy between the results for the two experiments. For this reason it was decided to repeat Sutherland's experiment with two modifications. First, discs as well as annuli were used and, second, the two control conditions in which both IF and TF were placed at the near and far positions were introduced. Thus the I_nT_f and I_fT_n conditions for annuli were the same as those used by Sutherland.

Apparatus. The inspection and test patterns were drawn in black on white art paper glued to masonite boards 25 in. and 10 in. square. The boards were mounted firmly on heavy chemical stands and supported from the top and bottom centres. The large figures were 10-in. diameter and the small figures 4-in. diameter. The thickness of the contours of the large annuli were $\frac{5}{32}$ in. and of the small annuli $\frac{1}{16}$ in. The fixation points which were at the centres of the squares were $\frac{5}{16}$ and $\frac{1}{8}$ -in. diameter and the contours of the figures were 1.875 and 0.75 in. from the centres of the fixation points.

Three tables were arranged, one for the far 10-in. figures, another for the near 4-in. figures and a third on which was mounted a head rest. The distances from the eye to the large and small figures were 1.44 and 57.6 in. respectively so that both sizes and distances were in the ratio 2.5:1 and the visual angle subtended at the eye was about 4° .

All observations were carried out with natural lighting from the windows of the room. The brightness of the near and far surfaces were approximately equal.

Subjects. There were eight groups of 12 subjects drawn from undergraduate classes in psychology.

Procedure. The procedure, including the instructions, was essentially similar to that used in the first experiment with the modification that, in order to conform with Sutherland's procedure, binocular viewing was used throughout and the fixation period was shortened to 45 sec. Since with binocular viewing retinal disparity prevented the subject adjusting the distance of his eyes so that the two squares were apparently coincident, he was required to position his head in the rest so that the upper and lower edges of the squares were apparently coincident. Due to retinal disparity the right and left edges of the far surface appeared to extend beyond the same edges of the near surface so that the subject was required to position his head laterally until the two visible strips of the far square were equal in width. The distance between the two squares and the head rest were regularly checked with a surveyor's tape.

Following fixation of the far pattern the near pattern was placed in a marked position on the table. After fixating the near pattern this was quickly removed by one experimenter and the far pattern placed in a marked position on its table. When both patterns were at the same distance one was removed by one experimenter and the second, held in readiness by another, was placed in position. This procedure reduced delay in changing patterns.

Results. The results for Experiment II are shown in the lower half of Table I, the entries again referring to the direction of changes, if any, between fore-test and after-test. The chi-squared test has again shown that the total obtained frequencies depart significantly from a pattern of equal frequencies in the three categories.

The results for this experiment are by no means as definite as in Experiment I. In the case of the annuli Sutherland's (1954) results are largely confirmed for the I_nT_f condition, eight of the entries being l , four e and there were no s entries. This result, however, has not been confirmed in the case of the discs where the frequencies tend to be evenly distributed. Under the opposite condition (I_fT_n) there is a tendency for s and e entries to predominate for both forms of stimulus figure.

An exact probability analysis (Myers, 1958) of the frequency data in Table I has indicated that, for the discs, none of the six differences between pairs of conditions is significant at less than a probability of 0.05, the probabilities ranging from 0.05 to 1.00. The probabilities are shown in the lower half of Table II. The probability of the difference between I_nT_f and I_fT_f (0.06) approaches the accepted lower level of significance. For the annuli the differences between I_nT_f and I_nT_n , I_nT_f and I_fT_n , I_nT_n and I_fT_n , I_fT_n and I_fT_f are significant. There is a significant difference between I_nT_f and I_fT_n conditions for annuli. Under the first there is a predominance the

of *l* and *e* entries, and under the second condition a greater number of *s* entries. Sutherland's results, therefore, are confirmed.

DISCUSSION

The principal result from these two experiments is the difference in frequency of apparent shrinkages and expansions of TF for the large and small figures. In the case of both the disc and annulus figures used in Experiment I the data shows that either no after-effect occurred, or that it took the form of an apparent shrinkage of TF. An apparent expansion of TF occurred in only 7.5 per cent. of the cases. With the large figures used in Experiment II the results are by no means as conclusive as those for the smaller figures. Whereas with the annulus figures there was a tendency for an expansion to be reported under some conditions of distance relationship between IF and TF, this tendency was absent when the disc figures were used. Sutherland's (1954) observation that with IF near and TF far and both figures subtending the same visual angle TF tends to expand following fixation has been largely substantiated using the same type of annulus figure. It is quite clear, however, that the size of the figure is an important determinant of the FAE the reverse tending to occur with the smaller figures.

Some light is thrown on the results reported here by a consideration of the time error effect (TE) which is characteristic of successive judgements in psychophysical experiments. When two physically equal stimulus figures are judged in succession, the second is judged larger or smaller than the first, the type of judgement depending upon numerous factors including the time interval between successive judgements. This characteristic effect is the TE. The error is traditionally positive when the second stimulus is judged as smaller than the first and negative when it is judged as larger. There are many similarities between the FAE and TE. Both involve the judgement of one figure following the presentation of another. Although, in the case of the FAE, the fixation time of the first figure is usually protracted, the FAE is known to occur with the very brief exposure times used in experiments on TE (Köhler and Wallach, 1944). In the study of TE, however, the stimulus presented second is compared to that presented first whereas in the study of the FAE the second presented stimulus figure is usually judged relative to a simultaneously presented CF falling on a different retinal area. It is well established that the direction of TE (positive or negative) is determined by the position of the stimuli in relation to the stimulus series. When the stimuli are from the large end of the series the second of the two stimuli tends to be judged larger (negative TE) than the first; when they are from the small end the second tends to be judged smaller (positive TE). Hollingworth (1909) referred to this series effect as the "Central Tendency Effect." More recently Watson (1957) in a study of TE in successive size judgements of circular stimulus figures has shown that for the small figures (about 3 in. diameter) TE is positive and for the large figures (10 to 24 in. diameter) TE tends to be negative. Watson demonstrated a decreasingly positive and an increasingly negative TE as the stimulus figures increased in diameter. Of greatest interest to the present experiments, however, is the range of stimulus sizes in which the change from positive to negative TE occurred. The graphed results (Watson, 1957, p. 564) show that roughly in the range 5 to 11 in. there was a tendency for the TE to change direction. The stimulus figures used in Experiment I are of that order of size in which Watson (1957) has shown that TE is positive, i.e. the second figure tends to be judged smaller than the first. The stimulus figures used in Experiment II and by Sutherland (1954) approximate to the range of sizes in which the change over from positive to

negative TE has been found to occur, and since those figures are at the upper end of this range it would be expected that a greater number of negative TEs would be found.

There is a compelling similarity between the data reported by Watson for TE with stimulus figures varying in size and those found here for the FAE with figures which fall within the same range of stimulus magnitude. Two hypotheses seem possible.

The stimulus figures used in the present experiments are peculiar in that the IFs and TFs were of the same shape and type and subtended the same visual angle. Although the subject was required to make a simultaneous comparison of TF and CF it is not inconceivable that the true FAE has been obscured by the TE. It has already been noted that the FAE under the conditions of coincidence of IF and TF is minimal so that it is possible that this slight after-effect could be obscured by a large TE when the successively observed figures were similar. The FAE is confined to that retinal area stimulated and drops off considerably beyond the boundaries of that area (Köhler and Wallach, 1944). Although there is no direct evidence available it is generally assumed that TE, which does not require steady fixation of the stimulus figures, is not so restricted to the stimulated retinal areas but is a general feature of successive judgements.

An alternative hypothesis is that the FAE and the TE in successive size judgements are basically the same phenomenon. Although, as already pointed out, the FAE with small IFs and TFs is an apparent shrinkage of TF the writers have evidence yet to be completed and reported that with much larger figures the FAE is reported as an apparent expansion. The progressive change in TE from positive to negative with increase in figure size as found by Watson (1957) may be an after-effect from short fixation and arising from the same retinal and neural processes as the FAE. A crucial experiment would be one similar to Watson's in which the figure presented second stimulates a different part of the retina to the first. If the effect noted by Watson is essentially an FAE then it should not occur if the second stimulus figure falls on a part of the retina not stimulated by the first.

There are further features of the results which warrant attention. In all previous experiments concerned with this problem only the I_fT_n and I_nT_f conditions have been investigated. In Experiment I there was no difference between the frequency patterns of judgements under these conditions and under those in which both figures were either near or far. In Experiment II for the annulus figures the greater frequency of expansions for the I_nT_f condition is similar to the frequency found when both figures were at the far position. With this control included it would be difficult to attribute the expansions to the effects of apparent size.

In both Experiments I and II there was a significant difference between the judgement frequency patterns for annuli and discs under the I_nT_f condition. This suggests that for certain distance relationships between IF and TF the type of figure employed may be an important determinant of the FAE. Further, in Experiment II significant differences between frequency patterns has occurred between four pairs of distance relationship conditions in the case of the annulus figures. These data indicate that there may be interaction effects between distance relationships of IF and TF and the type of figure used.

Generally the results from these two experiments suggest that the apparent size of the stimulus figures is not a critical determinant of the direction of the FAE. Rather it appears that the sizes, distance relationships and types of figures determine the nature of the after-effect.

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REFERENCES

- HOCHBERG, J. E., and BITTERMAN, M. E. (1951). Figural after-effects as a function of the retinal size of the inspection figure. *Amer. J. Psychol.*, **64**, 99-102.
- HOLLINGWORTH, H. L. (1909). The inaccuracy of movement. *Arch. Psychol.*, **2**, 1-87.
- KÖHLER, W., and WALLACH, H. (1944). Figural after-effects: an investigation of visual processes. *Proc. Amer. Phil. Soc.*, **88**, 4.
- MC EWEN, P. (1959). Figural after-effects, retinal size and apparent size. *Brit. J. Psychol.*, **50**, 41-7.
- MYERS, J. L. (1958). Exact probability treatments of factorial designs. *Psychol. Bull.*, **55**, 59-61.
- POLLACK, R. H. (1958). Figural after-effects: quantitative studies of displacement. *Aust. J. Psychol.*, **10**, 269-77.
- PRENTICE, W. C. H. (1947). The relation of distance to the apparent size of figural after-effects. *Amer. J. Psychol.*, **60**, 617-23.
- PRENTICE, W. C. H. (1950). On the size of figural after-effects with varying distances. *Amer. J. Psychol.*, **63**, 589-93.
- SAGARA, M., and OYAMA, T. (1957). Experimental studies on figural after-effects in Japan. *Psychol. Bull.*, **54**, 327-38.
- SUTHERLAND, N. S. (1954). Figural after-effects, retinal size and apparent size. *Quart. J. exp. Psychol.*, **6**, 35-44.
- WALTHALL, W. J. (Jun.) (1946). The Köhler effect. *Amer. J. Psychol.*, **59**, 152-5.
- WATSON, W. A. (1957). Contrast, assimilation, and the effect of central tendency. *Amer. J. Psychol.*, **70**, 560-8.

HAS APPARENT SIZE BEEN TESTED AS A FACTOR IN FIGURAL AFTER-EFFECTS?

BY

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This experiment was designed to demonstrate that the effect of apparent size on the FAE has not been adequately tested. The subjects were run under binocular and under monocular viewing conditions. Binocular vision gave essentially the same results that had been obtained in previous binocular investigations. Monocular vision gave different results. It was concluded that the effect of apparent size on the FAE has not been demonstrated.

INTRODUCTION

It has been asserted that both retinal size and apparent size contribute to the figural after-effect (FAE). Prentice (1950) and Hochberg and Bitterman (1951) claim that retinal size is the determining factor in this effect. Sutherland (1954), on the other hand, claims that the conditions set up by Prentice and by Hochberg and Bitterman were optimal for determination by retinal size, and Sutherland asserts that apparent as well as retinal size may contribute to the FAE.

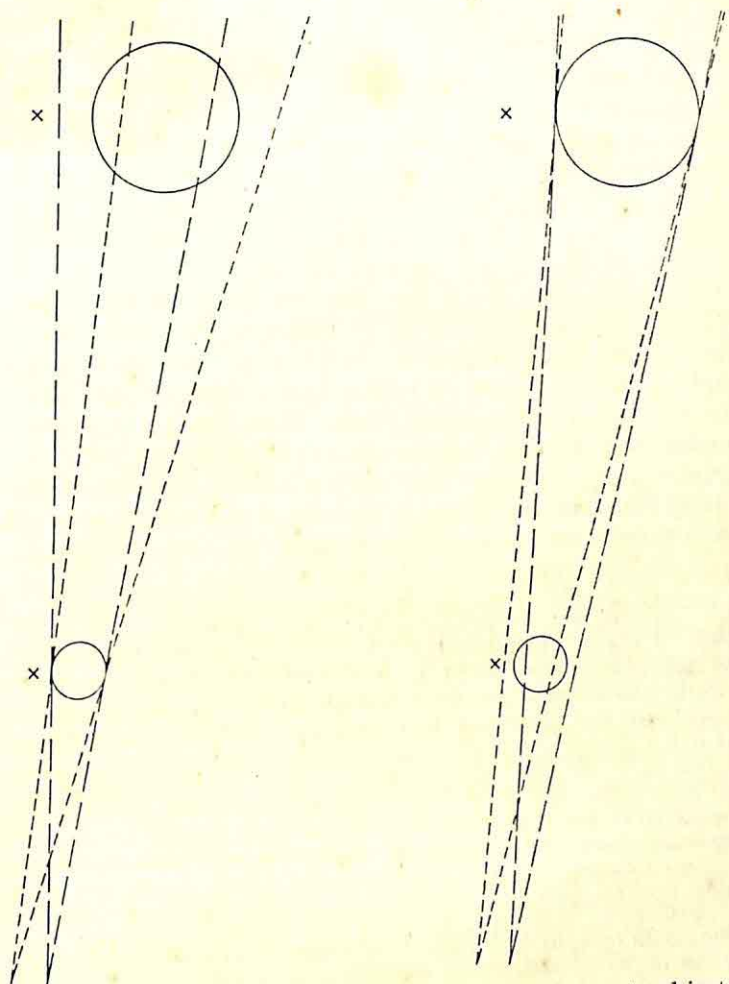
The evidence offered for these hypotheses is based on experiments in which Inspection (I) and Test (T) figures are of different sizes and are set up at different distances from the subject. Prentice's work consists of one-half an experiment suggested by Thouless. Thouless' solution to the problem of the relative importance of retinal and apparent size for the FAE consisted in presenting a large distant I-figure that would intercept a smaller retinal angle at the near plane of the T-figure than the small T-figure itself and in presenting a small close I-figure that would project a larger retinal angle at the far plane of the T-figure than the large T-figure itself. (I- and T-figures of course were aligned so that their centres fell on a straight line from the subject's point of regard. The T-figure was compared with an identical figure in the other half of the subject's field of view.) Prentice repeated the first half of this experiment; Hochberg and Bitterman, the second half. Findings support the claim that retinal size determines the FAE.

Sutherland criticizes the experiments of Prentice and of Hochberg and Bitterman for their use of dimensions that maximize a retinal effect. In Sutherland's experiments, the retinal projections of I- and T-figures are asserted to be the same. (He gives the distance used from fixation point to circle on the near card as 0.75 in. rather than as the 0.9 in. that would seem to be necessary to secure identical projections.) Using these projections, Sutherland finds that FAEs are affected by apparent size. Moreover, when Sutherland repeated the Hochberg and Bitterman study with his figures (by moving his small circle towards the subject), he obtained essentially their results. Therefore, concludes Sutherland, apparent as well as retinal size may determine the FAE.

This paper is not intended to dispute Sutherland's thesis. His method, however, seems to contain a possible source of error, as do also the methods of Prentice and Hochberg and Bitterman. For, in these experiments, binocular vision was used. Now, when vision is binocular, I- and T-figures, each at different distances, cannot be aligned so that their centres fall on the same line of sight to both eyes. Thus, retinal projections of I- and T-figures are not identical.

When I- and T-figures are aligned at different distances so that the projections from these figures are, in fact, the same to a given point and when this point is the midpoint of the interocular distance, the following situations ensue:

FIGURE 1



Illustrative sketch of visual projections from two points to objects at a distance.

In the left diagram, fixation is at the distance of the close object; in the right diagram, fixation is at the distance of the far object.

These diagrams are not drawn to scale.

In this case, the projections from I- and T-figures deviate as little as possible from the lines of sight to both eyes. It would, of course, be possible to set up the figures so that their centres are aligned from one eye, but such an arrangement would result in misalignment from the other eye. Minimal deviation from the lines of sight of both eyes is obtained by the procedure shown in Figure 1. In the diagram to the left where the I-figure is close, the T-figure is set up so that its two retinal projections deviate from each other as little as possible; in the diagram to the right where the I-figure is distant, the T-figure again is set up so that its two retinal projections deviate from each other as little as possible. That is, since I- and T-figures are set up so that

their projections are the same to the midpoint of the interocular distance, the deviation of these projections from the lines of sight to both eyes is minimal.

It is clear that for binocular vision, identical projection of I- and T-figures is not possible. Furthermore, under these conditions, identical projection even to one eye cannot occur. Whether monocular inspection and monocular testing were to be carried out by the same or by alternate eyes, the T-figure retinal projection would differ from that of the I-figure. This follows since the centres of both figures are not aligned with either central line of sight.

Therefore, whether binocular or monocular vision is used, this method does not seem to represent an optimal condition for ascertaining the effect of apparent size on the FAE. Now, suppose that exactly this method were to be used for both binocular and monocular vision. Clearly, if different findings were to result for binocular and monocular vision, it could not be asserted that apparent size has been demonstrated to be a factor in the FAE.

For, if apparent size is the factor that determined Sutherland's results (obtained binocularly), then similar results should be obtained with monocular vision. The monocular findings might be made with one eye or with alternate eyes used for inspection and testing. The use of only one eye, however, permits "retinal" as well as cortical effects to function in the FAE. Therefore, in the present experiment, one eye was used for inspection and the other eye, for testing.

The purpose of this experiment was to determine whether results do differ when monocular rather than binocular vision is used and thus to ascertain whether apparent size has been demonstrated to be a factor in the FAE.

METHOD

Apparatus. A head-rest was attached to one end of a 12 × 3 ft. table. A wooden frame was built at the other end of the table, 12 ft. from the location of the subject's eyes when his head was in position in the head-rest. This frame was designed to hold a 22 × 28 in. card. An identical frame was placed 57.6 in. from the location of his eyes, and this second frame was hinged to the table top so that it could be dropped forward or lifted upright quickly.

Brown paper was arranged up to a height of 6 ft. about 2 ft. away on each side of the table and around the end away from the subject. Artificial illumination was used and was adjusted to be equal for both sides of the apparatus.

The cards used were white cardboard 22 × 28 in., and all figures were drawn in indian ink. Two I-cards were used. On one, to be used in the frame 12 ft. from the subject, a 10-in. outline circle (0.1 in. thickness) was drawn 2.25 in. to the side of a cross in the centre of the card. On the other, to be used in the frame 57.6 in. from the subject, a 4 in. outline circle (0.04 in. thickness) was drawn 0.9 in. to the side of a cross in the centre of the card.

Two T-cards were used. On one, to be presented in the frame 57.6 in. from the subject, two 4-in. outline circles (0.04 in. thickness) were drawn 0.9 in. to the sides of a cross in the centre of the card. On the other, to be presented in the frame 12 ft. from the subject, two 10-in. outline circles (0.1 in. thickness) were drawn 2.25 in. to the sides of a cross in the centre of the card. (These dimensions are the same as Sutherland's except that the distance from fixation point to the circle on near card is 0.9 in.)

When these cards were in place, the crosses were directly aligned with the midpoint of the interocular distance so that the circles on either side of the crosses would cut the same angle to this interocular midpoint.

Subjects. There were 80 men and 80 women from introductory psychology courses at the University of California, Berkeley. Each subject was used only under one condition.

Procedure. For Groups I and III, this was a replication of Sutherland's experiment. The head-rest was positioned so that the crosses on both I- and T-cards were aligned with a point midway between the subject's eyes. The subject was seated and told that after he had fixated the cross on the I-card (it was in position) for 45 sec., he would be shown another card, either closer to him (Group I) or farther away (Group III) than the card in position. He was told that he should fixate the cross in the centre of the second card

and say which, if either, of the two circles on that card appeared larger. The subject was asked to close his eyes for a second while the second card was being presented.

When the subject had placed his chin firmly on the head-rest, the experimenter said, "look at the cross," clicked the stop-watch, and timed him for 45 sec. The experimenter then said, "Close your eyes . . . Open them," while raising the T-card (Group I) or dropping down the I-card (Group III). He then said, "Look directly at the cross . . . Does either circle appear larger?" The subject's answer was recorded.

I-figures were presented on the left and on the right by inverting the I-card.

This procedure was followed for Groups II and IV except that for these groups, the subject held a 3×5 in. card over the eye that was not on the side of the I-figure while he inspected the I-card with the uncovered eye (both eyes open). While the T-card was being presented, the subject closed his eyes for 1 sec. and shifted the 3×5 in. card to cover the eye previously used for fixating the I-figure. Thus the T-card was presented to the eye that had been covered during inspection. Otherwise, Group II followed the procedure for Group I, and Group IV followed the procedure for Group III.

RESULTS AND DISCUSSION

No significant differences were found between right and left presentations of the I-figure, and no significant sex differences were found. Therefore, the data were grouped as in Table I.

TABLE I
RESPONSES OF SUBJECTS WHEN PRESENTED WITH T-CARD

	Group			
	I	II	III	IV
	large distant I-figure		small near I-figure	
	binocular	monocular	binocular	monocular
T-figure on same side as I-figure reported larger	10	23	26	31
No difference reported	5	9	7	2
T-figure on side opposite I-figure reported larger	25	8	7	7

In order to test the significance of results for binocular vision, Group I was compared with Group III. In order to test the significance of results for monocular vision, Group II was compared with Group IV. For each comparison, three chi-squares were computed. One chi-square was calculated on the basis of all three categories of response; a second chi-square was calculated with two categories of response, those subjects reporting no difference in the test figures being equally divided between the other two categories; and a third chi-square was calculated with two categories of response, those subjects reporting no difference in the test figures being discarded. These results are presented in Table II.

It is clear that all chi-squares calculated for binocular vision are significant, whereas all chi-squares calculated for monocular vision are non-significant. Thus, results for binocular vision are essentially the same as Sutherland's, but for monocular vision, results differ from Sutherland's.

Although binocular tests might be taken to indicate that apparent size is a factor in figural after-effects, this supposed effect of apparent size is not demonstrated when monocular tests are run under otherwise identical conditions. Now, there seems to be no reason that, under these conditions, apparent size would function in figural

after-effects for binocular and not for monocular vision. Therefore, it is concluded that some factor or factors other than apparent size may be responsible for the findings with binocular vision, that is, for Sutherland's results and for the results found in the present experiment with Groups I and III.

TABLE II

CHI-SQUARES FOR DIFFERENCES BETWEEN GROUPS RUN BINOCULARLY AND FOR DIFFERENCES BETWEEN GROUPS RUN MONOCULARLY

	<i>Groups I vs. III</i>	<i>df</i>	<i>p</i>	<i>Groups II vs. IV</i>	<i>df</i>	<i>p</i>
Assumption*						
A	20.57	2	>0.001	5.71	2	0.05 < >0.10
B	14.49	1	>0.001	1.33	1	0.20 < >0.30
C	17.19	1	>0.001	0.55	1	0.30 < >0.50

* A: 3 categories of response.

B: 2 categories of response ("No difference" category divided equally between other two categories).

C: 2 categories of response ("No difference" category discarded).

Except for the fact that Groups II and IV viewed the I- and T-figures monocularly, experimental conditions for these groups duplicated the conditions for Groups I and III. As already stated, this procedure did not provide optimal conditions for monocular testing of the effect of apparent size on the FAE. Thus, failure to find such an effect may not indicate its non-existence.

REFERENCES

- HOCHBERG, J. E., and BITTERMAN, M. E. (1951). Figural after-effects as a function of the retinal size of the inspection figure. *Amer. J. Psychol.*, **64**, 99-102.
- PRENTICE, W. C. H. (1950). On the size of the figural after-effect with varying distances. *Amer. J. Psychol.*, **63**, 589-93.
- SUTHERLAND, N. S. (1954). Figural after-effects, retinal size, and apparent size. *Quart. J. exp. Psychol.*, **6**, 35-44.

RETINAL SIZE AS A DETERMINER OF THE DIRECTION OF SIZE DISTORTION IN FIGURAL AFTER-EFFECTS

BY

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When dealing with size distortions, the direction of the distortion is determined by the retinal size of the T-figure with respect to the retinal size of the I-figure. When the T-figure is retinally smaller than the I-figure it is distorted toward appearing even smaller; when it is retinally larger than the I-figure it is distorted toward appearing even larger; and when the two are retinally equal, there is no distortion. If the T-figures are too much larger or smaller than the I-figure, the distortion ceases. The traditional distance paradox is observed where the distance is defined retinally. The apparent size of the figures is irrelevant to a prediction of what sort of distortion takes place. Sutherland's hypothesis that apparent size determines the direction of distortion when retinal sizes are equated is not supported.

Köhler and Wallach (1944) have shown that if an observer fixates a figure, called the inspection or I-figure, for a given length of time, certain systematic distortions will be reported in many subsequent figures which are observed. One of these distortions in the subsequent figure, called the test or T-figure, is that of size. According to these authors, this distortion is restricted to the cortical area close to that stimulated by the I-figure. If we are given, for example, an I-figure of a circle and a T-figure of two equal sized circles which are larger than that I-figure, one surrounding the area previously occupied by the I-figure and the other at some distance from it, then the surrounding T-circle will look larger than its partner. If the T-circles are smaller than the I-circle, then the one which falls within the previously stimulated area will look smaller than its partner. If the T-circles are too much larger than the I-circle, or too much smaller, then no distortion will take place and the two T-circles will appear equal in size.

During their investigations, Köhler and Wallach (1944) situated the I- and T-figures at the same distance from the observer, thus making it impossible to tell exactly what determined this cortical "size." There are two alternatives here, apparent size and retinal size. By apparent size we mean size constancy size or the fact that, for example, a 2-in. circle appears to be the same size whether it is 2 ft. or 10 ft. from the observer, and that it looks its actual size—2 in.; by retinal size we mean the size of the retinal image, keeping in mind the fact that at 10 ft. the circle casts a smaller image on the retina of the observer's eye than it does at 2 ft. These two alternative hypotheses would lead to equivalent results when both the I- and T-figures are at the same distance from the observer, since at any given perceived distance, apparent and retinal size will be perfectly correlated, i.e. larger objects will cast larger images.

Since Köhler and Wallach talked at great lengths about the cortical mechanisms involved in figural after-effects, it might seem that by "size" they would mean apparent size. Köhler, in other writings, has maintained that apparent size is a cortically given sensory fact so long as any depth information is available to the observer (Köhler, 1940), and such is the case in most figural after-effect studies. Hochberg and Bitterman (1951) have claimed, however, that a retinal size interpretation is what Köhler and Wallach intended. Actually the latter theorists cannot be pinned down to any one position due to the logical looseness of their model. The competing theory, that of Osgood and Heyer (1952) is likewise unspecific. Since

they speak in terms of the sensory projection fibres one might say that Osgood and Heyer would support the retinal size hypothesis, but this is mere conjecture. The question is, then, what "size" is the empirically correct one?

Prentice (1950), Hochberg and Bitterman (1951), and Terwilliger and Hastorf (unpublished), have all presented evidence in favour of the retinal size hypothesis. This was done by making the T-figures actually, hence apparently, larger (smaller) than the I-figure. But, by putting them farther from (nearer to) the subject than the I-figure they became retinally smaller (larger). Thus the situation was such that the retinal size hypothesis would predict one direction of distortion while the apparent size hypothesis would predict the opposite. The above studies reported that if the T-figure was retinally smaller than the I-figure, the T-figure falling in the previously stimulated retinal area was distorted toward appearing smaller than its equal sized partner even though it was apparently larger than the I-figure. Likewise the T-figure falling in the previously stimulated area appeared larger than its partner if the T-figure was retinally larger than the I-figure even though it was apparently smaller. Evidently the apparent size of the figures was of no importance.

There are, however, two studies in the literature which might be interpreted to show that apparent size also plays a role in determining the direction of the after-effect. The first of these studies, done by Prentice (1947), is virtually uninterpretable due to a methodological weakness in the design. An angle of about 30 degrees from the horizontal existed between the I-figure and the fixation point on the stimulus display card. A more recent study by Fox (1951) indicates that this type of situation will give rise both to strong after-effects of the type discovered by Gibson (1937) and also to Köhler-Wallach effects. These are regarded by many investigators (Fox, 1951; Seagram, 1957, 1958) as being distinctly different types of after-effects, hence the Gibson effects may not follow the same laws as do the size effects in question. Since it is impossible in the Prentice study to separate the Gibson from the Köhler-Wallach effects, it is impossible to know exactly what the study does show. This study has also been criticized by Prentice himself in the light of his subsequent retinal size findings (1950).

The second study, done by Sutherland (1954), showed that if the retinal sizes of the I- and T-figures were equated but the apparent sizes varied by varying the distances between them and the observer, then distortion was in the direction predicted by the apparent size hypothesis. Thus, while the I- and T-figures were retinally equal, if the T-figures were apparently larger than the I-figure, that T-figure which fell in the previously stimulated retinal area would appear larger than its equal sized partner. This study is open to several criticisms. The first is that Sutherland's subjects showed a surprising response set, tending to report the T-figure falling in the previously stimulated area as larger no matter what experimental manipulation was performed on them. A second criticism is that there is some doubt as to whether a constant visual angle was maintained between the figures and the fixation point as the distance was varied. If this was not done, then these results may be due to this uncontrolled variable of retinal locus and not to the apparent size of the figures. A third and empirical criticism is that, in a study which was devoid of the above faults, Terwilliger and Hastorf (unpublished) were unable to replicate Sutherland's results. Indeed, they found a slight but insignificant tendency to see the T-figure as *smaller* when it was retinally equal to but apparently larger than the I-figure. These results are consonant with those reported by Köhler and Wallach (1944) when they used retinally and apparently equal I- and T-figures. They are also consistent with a retinal size hypothesis that assumes that when retinal sizes are equal, there will be no significant distortion of the T-figure.

The bulk of the results seems to indicate that retinal size is the important and probably the only determiner of the direction of the size figural after-effect. There seems to be some need, however, for a more thorough investigation of the phenomenon. The following study is intended to test the hypothesis that the direction of distortion is a function solely of the visual angle subtended by the T-figure in relation to that subtended by the I-figure. If the retinal angle of a T-figure falling in a previously stimulated area of the retina is larger than that of the I-figure, it will be seen as larger than an actually equal figure falling in a non-stimulated retinal area; if it is smaller then it will be seen as smaller than an equal sized figure falling in a non-stimulated area; and if it is retinally equal in size to the I-figure there will be no distortion. Such effects will be independent of the distance of the figures from the observer, the distance between the I- and T-figures and of the apparent size of the figures. It is expected that Köhler-Wallach size effects will follow a law of visual angle in much the same sense that negative after-images do. In other words, in the sense that "size" is retinally defined, Emmert's law applies to these effects as well as to after-images of the common sort.

METHOD

The apparatus consisted of a plywood wall into which was cut an opening through which the subjects could look comfortably with both eyes while seated. In order for each subject's head to be comfortably placed in an equivalent position, a formed head-piece such as that used on a common hand stereoscope was built around this opening. The opening could be opened or closed from the other side of the wall at the discretion of the experimenter. Behind the wall was a table approximately 10 ft. in length, nearly all of which could be seen from the opening. On the table, at distances of 4, 6 and 8 ft. from the eyes of the observer looking through the opening, were placed stimulus card

TABLE IA

Position of inspection figure	Near Inspection					
	2 in. circle at 4 ft. ($2^{\circ} 23'$) centre of circle 3 in. from fixation point					
	Left of fixation			Right of fixation		
Diameter of test figure	2 in.	3 in.	4 in.	2 in.	3 in.	4 in.
Distance of test figures: 4 ft.	$2^{\circ} 23'$ 3 in. none none	$3^{\circ} 35'$ 3 in. larger larger	$4^{\circ} 46'$ 3 in. larger larger	$2^{\circ} 23'$ 3 in. none none	$3^{\circ} 35'$ 3 in. larger larger	$4^{\circ} 46'$ 3 in. larger larger
6 ft.	$1^{\circ} 36'$ 4.5 in. smaller none	$2^{\circ} 23'$ 4.5 in. none larger	$3^{\circ} 11'$ 4.5 in. larger larger	$1^{\circ} 36'$ 4.5 in. smaller none	$2^{\circ} 23'$ 4.5 in. none larger	$3^{\circ} 11'$ 4.5 in. larger larger
8 ft.	$1^{\circ} 12'$ 6 in. smaller none	$1^{\circ} 48'$ 6 in. smaller larger	$2^{\circ} 23'$ 6 in. none larger	$1^{\circ} 12'$ 6 in. smaller none	$1^{\circ} 48'$ 6 in. smaller larger	$2^{\circ} 23'$ 6 in. none larger

TABLE IB

Position of inspection figure	<i>Far Inspection</i>					
	<i>4 in. circle at 8 ft. ($2^{\circ} 23'$) centre of circle 6 in. from fixation point</i>					
	Left of fixation			Right of fixation		
Diameter of test figure	2 in.	3 in.	4 in.	2 in.	3 in.	4 in.
Distance of test figures: 4 ft.	$2^{\circ} 23'$ 3 in. none smaller	$3^{\circ} 35'$ 3 in. larger smaller	$4^{\circ} 46'$ 3 in. larger none	$2^{\circ} 23'$ 3 in. none smaller	$3^{\circ} 35'$ 3 in. larger smaller	$4^{\circ} 46'$ 3 in. larger none
6 ft.	$1^{\circ} 36'$ 4.5 in. smaller smaller	$2^{\circ} 23'$ 4.5 in. none smaller	$3^{\circ} 11'$ 4.5 in. larger none	$1^{\circ} 36'$ 4.5 in. smaller smaller	$2^{\circ} 23'$ 4.5 in. none smaller	$3^{\circ} 11'$ 4.5 in. larger none
8 ft.	$1^{\circ} 12'$ 6 in. smaller smaller	$1^{\circ} 48'$ 6 in. smaller smaller	$2^{\circ} 23'$ 6 in. none none	$1^{\circ} 12'$ 6 in. smaller smaller	$1^{\circ} 48'$ 6 in. smaller smaller	$2^{\circ} 23'$ 6 in. none none

holders built to receive cards 30 in. in the horizontal direction by 10 in. in the vertical. The holders were such that cards could be slid easily in and out of them. The whole apparatus was illuminated from above by one 40 Watt fluorescent bulb situated 50 in. above the table in such a manner that each card holder received approximately the same amount of light and such that the light would be distributed evenly over the surface of each card.

The stimuli were line circles drawn in indian ink on white illustration board. The thickness of the contours was $\frac{1}{16}$ in. On each stimulus card was a cross drawn in indian ink to be used as a fixation point. This cross was located in the centre of the card, which was, in turn, placed in such a way that the subjects would fixate the cross while looking straight ahead. The I-figures consisted of a circle placed at one side or the other of the fixation point, with the centre of the circle on a horizontal line with the fixation point. The T-figures were pairs of circles, one on each side of the fixation point, both equidistant from it and with their centres in a line horizontal with the fixation point and passing through it.

The I-figure was either a 2-in. diameter circle 4 ft. from the observer or a 4-in. circle 8 ft. from him. Both subtended the same visual angle and both were drawn so that their centres subtended the same angle with the fixation point. At each distance, the I-circle could be to the right or to the left of the fixation point, thus making possible four different inspection conditions. The T-figures were pairs of 2-, 3- or 4-in. diameter circles at either 4, 6 or 8 ft. from the observer, making nine T-conditions in all. The centres of both the I- and T-figures subtended the same angle with respect to the fixation point in all conditions. There were, therefore, 36 different possible experimental conditions. Table IA and IB show the details of these as well as the predictions made for each.

In each cell is recorded in the following order:—

- (1) The visual angle of the T-circles.
- (2) The distance between the centre of each T-circle and the fixation point.
- (3) Distortion on the previously stimulated side if the retinal hypothesis is true.
- (4) Distortion on the previously stimulated side if the apparent hypothesis is true.

One hundred and eighty subjects in all were used. These were undergraduates of both sexes who were enrolled in the introductory psychology course. Each subject was run in two conditions which were in all respects identical for any given subject except for the distance of the I-figure. Thus each subject was run once in a Near Inspection condition and once in a Far Inspection condition, but in both of these the I-side, T-size and T-distance were the same. Half the subjects were given the Near Inspection condition first and half were given the Far Inspection condition first. Otherwise the subjects were assigned to conditions randomly.

The experimental session was run in the following way. When the subject arrived he was asked to seat himself and after placing his head in the head piece, to look through the opening in the wall. He was shown a 2-in. circle at 4 ft. and a 3-in. circle at 8 ft. simultaneously and asked to say "which circle is the larger." This condition was introduced to demonstrate that some degree of size constancy existed in the situation and thus, since retinal and apparent size were not perfectly correlated, that the apparent size hypothesis was getting a fair test. Note that the retinal and apparent sizes of these circles are opposite here—the retinally larger is apparently smaller.

After the size constancy judgement, the first of the figural after-effect measurements was made. The subject was told that he would see a card with a circle and a cross on it and that he was to fixate on the cross. He was told that he "could blink his eyes normally" but that he should try not to move them off the cross. This fixation was to last for 1 min. which was "a long, boring time" so he should be patient. He was told that at the end of the minute, the experimenter would close the opening and while it was closed he was to keep his head steady in the head piece. When the window opened again, he would see another card and would be asked to make a judgement about it.

The subject was then made to fixate the cross on an I-card for 1 min. at the end of which the opening was closed while the experimenter changed cards. When the T-figure was in place (this took about 5 sec.) the window was opened and the subject told to "fixate the cross and, while fixating, compare the two circles in size and tell me which looks larger." If the subject responded that they looked equal, he was told that he must pick one or the other using whatever means he chose. Following this the subject was put out to rest for 15 min. at the end of which time he was run in another figural after-effect study. Fifteen min. following that study, during which time he rested, the subject was brought back and made his second judgement in this situation. The conditions were the same for the second judgement as for the first with the exception that the I-figure was at the other distance.

RESULTS AND DISCUSSION

A look at the responses of the subjects in the size constancy judgements indicates that size constancy was operating in this situation. Only two out of the 180 subjects responded retinally, i.e. said that the 2-in. circle was larger than the 3-in. circle. Therefore we are safe in assuming that the situation was a fair test of the apparent size hypothesis since the retinal and apparent sizes could be varied independently of each other, thus allowing us to put the two in conflict and see which dominates the subjects' responses.

Table II shows the results for the main experimental conditions. It can be seen that when the T-circle was retinally smaller than the I-circle (groups 7, 8, 13, 14, 15, 16), the circle on the stimulated side of the fixation point was reported as smaller than its partner in all conditions. This held in spite of the fact that the apparent size hypothesis would have predicted either no effect or the opposite effect in all conditions when the I-figure was near to the subject. Thus in these conditions the retinal size hypothesis is demonstrated.

When the I- and T-figures subtended equal visual angles (groups 1, 2, 9, 10, 17, 18) there were no significant distortions, either in any single group or when the χ^2 's for each group were summed (Near Inspection, $\chi^2 = 0.73$; Far Inspection, $\chi^2 = 0.48$, d.f. = 3). Since the subject was forced to choose either "left" or "right," if no distortion was taking place each response should occur equally often. Since there are no significant differences these results are also in the direction predicted

TABLE II

<i>Test Circles</i>	<i>Responses</i>	<i>Near Inspection Figure (2° 23')</i>			<i>Far Inspection Figure (2° 23')</i>		
		<i>I Left</i>	<i>I Right</i>	<i>Exact p</i>	<i>I Left</i>	<i>I Right</i>	<i>Exact p</i>
2 in. T at 4 ft. 2° 23'	Left						
	Larger	4	4		2	4	
	(1) Right			0.675	8	6	0.314
3 in. T at 4 ft. 3° 35'	Larger	6	6				
	Left				6	1	
	(3) Right			0.005	4	9	0.029
4 in. T at 4 ft. 4° 46'	Larger	4	10				
	Left				5	4	
	(5) Right			0.185	5	6	0.500
2 in. T at 6 ft. 1° 36'	Larger	3	6				
	Left				2	7	
	(7) Right			0.035	8	3	0.035
3 in. T at 6 ft. 2° 23'	Larger	7	2				
	Left				5	4	
	(9) Right			0.325	5	6	0.500
4 in. T at 6 ft. 3° 11'	Larger	5	7				
	Left				4	1	
	(11) Right			0.016	6	9	0.152
2 in. T at 8 ft. 1° 12'	Larger	5	10				
	Left				0	7	
	(13) Right			0.0005	10	3	0.002
3 in. T at 8 ft. 1° 48'	Larger	1	9				
	Left				2	8	
	(15) Right			0.035	8	2	0.012
4 in. T at 8 ft. 2° 23'	Larger	2	7				
	Left				4	2	
	(17) Right			0.709	6	8	0.314
	Larger	8	8				

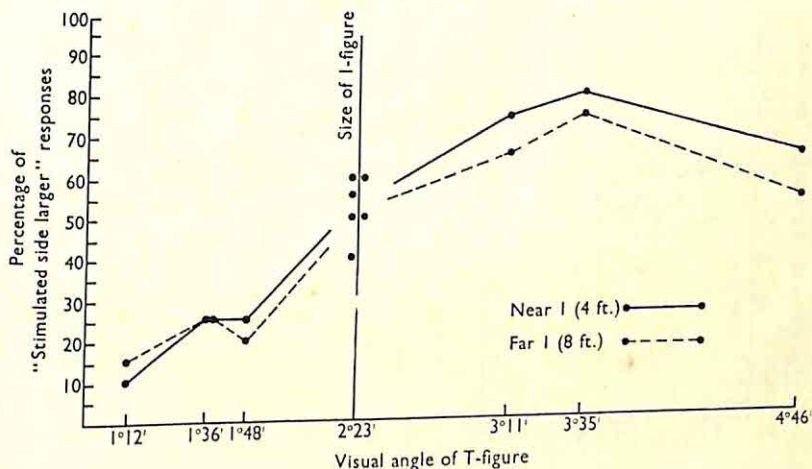
* 180 subjects used, each measured twice. For each measurement the T-figure and side of I-figure were constant. Only the distance of the I-figure varied.

by the retinal size hypothesis and are directly opposed to those reported by Sutherland (1954). While we do find a slight tendency for the subjects to report the stimulated side as larger (even though in some cases they should not, according to Sutherland) this does not reach the magnitude reported by him. Thus we must consider the retinal size hypothesis supported here too. There is no evidence in favour of the

apparent size hypothesis or of Sutherland's modification of it which states that apparent size will only show its effects when the I- and T-figures subtend the same retinal angle.

When the T-figure is retinally larger than the I-figure, once again the retinal hypothesis is generally supported (groups 3, 4, 5, 6, 11, 12). The subjects tend to report the stimulated side as larger than the non-stimulated side. There are, however, certain exceptions here. The 4-in. test circles at 4 ft. (groups 5, 6) do not show any significant distortions. The reason for this would seem to be quite simple. These circles are retinally the largest of any of the T-circles, large enough, apparently, so that they escape being distorted. We mentioned above that if the T-figure was much larger or much smaller than the I-figure no distortion would take place. This finding, originally reported by Köhler and Wallach (1944) and later verified by

FIGURE 1



Percentage of "Stimulated side larger" responses as a function of the visual angle of T-figure.

others (Pollack, 1958), is generally known as the distance paradox. Briefly described, the phenomenon is that, starting from equality, as the T-figure grows larger (or smaller) than the I-figure, distortion first increases and then decreases. The lack of significance in question can be most reasonably explained by assuming that this circle, being so large, is on the decreasing part of the function.

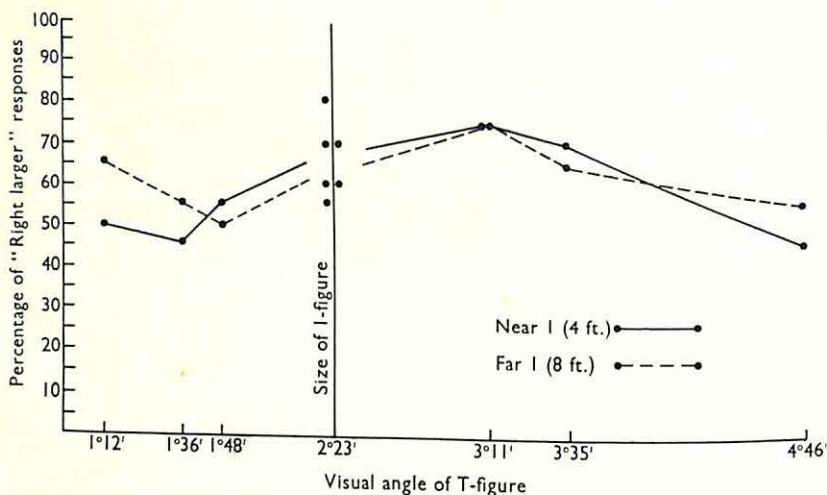
To illustrate this, the reader is referred to Figure 1 which shows the percentage of "stimulated side larger" responses as a function of the retinal angle of the T-figure. It will be seen that in that part of the curve representing T-figures retinally larger than the I-figure, one gets an increasing and then decreasing function reminiscent of the distance paradox findings.

As far as the other nonsignificant result (group 12) is concerned, we can give no reason for this except that the response bias which was noticeable in all conditions may have concealed the effect. There was a significant tendency for the subjects to report the right-hand circle as larger irrespective of the experimental condition. For all groups with the Near Inspection figure "right" was preferred to "left" as a response at the $p < 0.01$ level ($\chi^2 = 7.2$) as it was with all groups using the Far Inspection figure ($\chi^2 = 9.8$). Since the response "right" is not in favour of our retinal size hypothesis any more than is the response "left," this response bias can only conceal significant differences in favour of the hypothesis. The fact that in

most cases we did get significant results speaks all the more strongly in favour of our hypothesis.

An analysis of this response bias in terms of the retinal angle of the T-figures is shown in Figure 2. Although it appears that there may be fewer "right" responses as the difference in size between the I- and T-figures increases, this tendency is not significant in either the Near Inspection or the Far Inspection conditions (Near Inspection, $\chi^2 = 2.6$, $p > 0.10$; Far Inspection, $\chi^2 = 0.2$, $p > 0.50$). From the data collected, no reasons can be offered as to why this sort of response preference should have appeared. One suspicion is that it may be connected with the so-called half-meridional differences reported by other investigators (Brown, 1953*a*, 1953*b*). These are differences in the apparent sizes of objects which vary according to what part of the retina is stimulated. Thus, for instance, the same object may look larger when

FIGURE 2



Percentage of "Right Larger" responses as a function of the visual angle of T-figure.

it falls in the left half of the retina than when it falls on the right half. There is, however, no evidence in the literature of any systematic differences of this sort over subjects.

Returning again to Figure 1, it is necessary to show that the functions for the near I-figure and for the far I-figure are not significantly different, since this is what the retinal size hypothesis would predict. A McNemar test for the significance of changes (McNemar, 1955) indicates that there was no significant change of the subjects' responses from the Near to the Far Inspection condition ($\chi^2 = 1.2$). Therefore the retinal size hypothesis is supported in this point too. There are no significant differences even though the apparent size hypothesis would predict that the two groups would produce completely opposite results.

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REFERENCES

- BROWN, K. T. (1953*a*). Methodology for studying figural after-effects and practice effects in the Müller-Lyer illusion. *Amer. J. Psychol.*, **66**, 629-34.

- BROWN, K. T. (1953*b*). Factors affecting differences in apparent size between opposite halves of a visual meridian. *J. opt. Soc. Amer.*, **43**, 464-72.
- FOX, B. (1951). Figural after-effects: "satiation" and "adaptation." *J. exp. Psychol.*, **42**, 317-26.
- GIBSON, J. J. (1937). Adaptation with negative after-effect. *Psychol. Rev.*, **44**, 222-43.
- HOCHBERG, J., and BITTERMAN, M. (1951). Figural after-effects as a function of the retinal size of the inspection figure. *Amer. J. Psychol.*, **64**, 99-102.
- KÖHLER, W. (1940). *Dynamics in Psychology*. New York.
- KÖHLER, W., and WALLACH, H. (1944). Figural after-effects: an investigation of visual processes. *Proc. Amer. Phil. Soc.*, **88**, 269-357.
- MCNEMAR, Q. (1955). *Psychological Statistics*. New York.
- OSGOOD, C., and HEYER, A. (1952). A new interpretation of figural after-effects. *Psychol. Rev.*, **59**, 98-118.
- POLLACK, R. H. (1958). Figural after-effects: quantitative studies of displacement. *Aust. J. Psychol.*, **10**, 269-77.
- PRENTICE, W. H. C. (1947). The relation of distance to the apparent size of figural after-effects. *Amer. J. Psychol.*, **60**, 617-23.
- PRENTICE, W. H. C. (1950). On the size of figural after-effect with varying distances. *Amer. J. Psychol.*, **63**, 589-93.
- SEAGRIM, G. N. (1957). A further examination of non-satiational figural after-effects. *Aust. J. Psychol.*, **9**, 20-32.
- SEAGRIM, G. N. (1958). Non-satiational figural after-effects supplementary report. *Aust. J. Psychol.*, **10**, 364-5.
- SUTHERLAND, N. S. (1954). Figural after-effects, retinal size and apparent size. *Quart. J. exp. Psychol.*, **6**, 35-44.

IS THE FIGURAL AFTER-EFFECT ACCOMPANIED BY CHANGES IN DISCRIMINATION?

BY

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In the figural after-effect one part of the visual field is apparently expanded relative to the rest, and it is suggested that the expansion might correspond to an improvement in discrimination. An experiment, in which subjects were required to judge the direction of curvature of lines after fixation of a straight line, showed no such improvement.

INTRODUCTION

Köhler and Wallach (1944) sum up much of their work on the figural after-effect (FAE) by the rule that contours of the test figure appear to recede from positions which were previously occupied by contours of the inspection figure. This recession of contours is local, affecting only that part of the visual field which is near the inspection figure contours.

The field in which the test line is seen seems to be expanded in the region of the inspection line relative to other parts of the field. The question arises whether discrimination is changed by this relative expansion of part of the field. Does the apparently increased distance correspond to an increase in the number of JNDs which go to make up that distance? This seems likely, for increased familiarity with a figure should make it easier to discriminate that figure from others like it. Moreover, Deutsch (1951) found an analogous effect in hearing; stimulation of one ear for 4 min. by a tone of 1,000 c.p.s. caused a reduction of 47 per cent. in DL for pitch at that frequency, as against 2 per cent. when the stimulation had been at 800 c.p.s. The aim of the present experiment was to find whether this improvement in discrimination accompanies the visual FAE.

ESTIMATION OF EXPECTED MAGNITUDE

If the FAE is accompanied by a local improvement in discrimination, it should be possible to determine from data on the FAE the magnitude and duration of the change in discrimination which would be expected.

The magnitude of the change can be obtained from the curve showing the variation of FAE with distance between inspection and test contours. If the real distance between the position of a test figure and the position of the previously exposed inspection line is D , and the measured amount of after-effect is A , the apparent distance between the positions is given by $D + A$. Then the ratio (Average JND after inspection)/(Average JND before inspection) is given by $D/(D + A) = 1/(1 + A/D)$. A/D is the slope of the after-effect curve near the origin, where the after-effect is approximately proportional to the distance.

From data by Hammer (1949) and our unpublished data, A/D is found to be approximately 0.07 and 0.13, so that an estimate of 0.1 seems not unreasonable. This would correspond to about 10 per cent. reduction in JND.

For a 10 per cent. reduction in JND, we may estimate the improvement in performance by considering the normal sigmoid frequency-of-seeing curve; the curve representing here the percentage of correct judgements when a subject is asked to say whether a particular curve is concave left or concave right. The reduction in JND gives an improvement which is equivalent to a shift to a new sigmoid curve of

10 per cent. smaller standard deviation; that is, if a figure differs sufficiently from the straight to be correctly judged x per cent. of the time, the same figure will now be correctly judged more often because it lies further out on the new sigmoid. The difference in height of the two sigmoids for each particular departure from the standard is the expected improvement in percentage of correct judgements, and this is maximal at 2 per cent. with an average of correct judgements for both curves of approximately 80 per cent.

The above estimate of magnitude is for tests made within a few seconds after the end of the inspection period. The FAE falls off rapidly at first and then more slowly, and the change in discrimination would be expected to follow the same course in time. Thus for a 15 sec. inspection period the FAE would be halved in about 11 sec. (Estimated from data by Ikeda and Obonai reported by Sagara and Oyama (1957).)

EXPERIMENTS

(i) It is necessary to compare discrimination in a condition which would produce an FAE with discrimination in a similar condition which would produce no FAE. Thus:—

- Condition I. Fixation on a vertical straight line;
 Flashed vertical straight line;
 Flashed test figure, slightly curved, subject judges whether it is curved concave to left or right.
- Condition II. Fixation on a horizontal straight line;
 Flashed vertical straight line;
 Flashed test figure as before.

It would be expected that fixation on the vertical straight line (which is in the same position as the test line) will cause an FAE which exaggerates the apparent curvature of the test line, while fixation on the horizontal straight line will have no such effect. The flashed vertical straight line is inserted to counter the objection that merely seeing the straight line may be enough to improve the discrimination; the line is seen in both conditions.

To check whether the FAE is present, we add:—

- Condition III. Fixation on a vertical *curved* line;
 Flashed test figure (also curved), subject judges whether it is curved concave left or right. The curves are varied right or left in accordance with the method of serial exploration.

This measures the after-effect of inspection of a curved line, and a positive result would confirm that the FAE occurs in this experimental set-up. To test whether inspection of a straight line produces a similar FAE a comparison curve would be needed, so changing the situation; this test is, however, redundant in this context; were it to fail, the argument leading to expectation of improved discrimination would be invalid so that the main experiment need not have been made, but were it to succeed it would not help to show that discrimination has improved. Nevertheless, it would be expected to succeed, for Köhler and Wallach (1944) state that the magnitude of the after-effect is not altered by an inter-change between test and inspection figures; that is, inspection of a straight line should have the same effect on a curve as inspection of the curve has on the straight.

(ii) The figures were projected by a two-channel tachistoscope onto a beaded screen on which a bright fixation spot was always projected. Background illumination was provided by a red light dimmed so that the projected figures were seen in clear contrast.

The subject sat in front and to one side of the fixation spot, then to the subject was told to keep his eyes as well as he could on the fixation spot, then to judge whether the curved line shown was concave left or concave right. If in doubt, he was to make the best guess he could, as he would be right more often than not. This last was secured by selecting for each subject, on the basis of a preliminary run, an optimum pair of test curves to give a score of about 80 per cent. correct judgements, this being the optimum as calculated above.

All figures were single black lines 40 cm. long and 3 mm. wide placed centrally on a white disc 70 cm. diameter. The figure was projected so that the line passed through

the fixation spot, which was bright and of 7 mm. diameter. The figures were 1.7 m. from the subject's eyes.

There were three kinds of figure: a vertical straight line, a horizontal straight line, and a vertical curved line, the ends being displaced to right or left by 0.03, 0.07, 0.12, 0.24, 0.44, 0.84, 1.62, 3.12, 5.7 mm. giving a logarithmic scale of curvature.

For condition III, the 5.7 mm. curve, L or R, was used as inspection figure and most of the others used as test figures. For conditions I and II the straight lines were used as inspection figures and the test curves selected as above were either 0.44 L and R or 0.84 L and R.

(iii) Ten subjects were used, men and women between 18 and about 35 years old. Each subject had five sittings at each of which two runs were made in each of conditions I and II. FAE was measured in one run added to the last sitting.

Each run consisted of 22 inspections followed by judgements, in this sequence: 15 sec. inspection figure, $\frac{1}{2}$ sec. blank, 1 sec. vertical straight, $\frac{1}{2}$ sec. blank, 1 sec. test figure, 4 sec. blank during which the judgement was made; repeat to make up 22 judgements of which the first two were discarded. Five min. rest before starting another run.

In each sitting the order of conditions I and II was randomized, and in each run the sequence of left and right curves was randomized, except that there were ten of each in a run and that any sequence containing more than four successive rights or lefts was rejected.

For two subjects, numbers 9 and 10, the above scheme was varied by leaving out the flashed vertical straight line, showing: 15 sec. inspection figure, 2 sec. blank, 1 sec. test figure, and 4 sec. blank during which the judgement was made.

RESULTS

The results of the experiment are shown in Table I.

TABLE I

	Subject No.	1	2	3	4	5	6	7	8	9	10	Total	Pass.
Correct judgements of direction of curvature.	Condition I	157	120	170	155	159	146	134	148	155	149	1,493	2,000
	Condition II	156	132	160	134	166	164	141	144	151	156	1,504	2,000
FAE: Average curvature in the same direction as the I-figure for transitions L to R or R to L (mm.)		1.9	0.95	1.24	1.9	1.87	0.95	0.95	0.95	1.83	1.62	Mean 1.32	

The difference between conditions I and II in the proportion of correct judgements is small, giving $\chi^2 = 0.16$ for 1 d.f. Little difference is made to χ^2 by taking separately the two subjects who were not shown the flashed vertical line, and half of the subjects favoured each of the conditions.

The value obtained for χ^2 is not significant. Moreover, if we assume the improvement in discrimination to be 10 per cent. as estimated above, then the frequencies of correct judgements obtained in the two conditions of the experiment would each be a little more than one standard error from the mean, so that the combined result is significantly different from that expected; it is unlikely that there is any difference in discrimination of this magnitude.

Measurement of the FAE gives results comparable with those on which the estimate of change in discrimination was based. The curvature for changeover from L to R and R to L judgements was calculated by interpolation and is presented in Table I. It can be seen that all subjects showed an FAE and that the average FAE indicates a curve with ends displaced by 1.33 mm., approximately 23 per cent. of the curve used as an inspection figure.

CONCLUSIONS

The experiment shows that a period of fixation on a straight line does not lead to an improvement in discrimination of the direction of curvature of lines which are

nearly straight. Because it is shown that the same period of fixation of a curve in the same conditions gives an FAE, it seems that the FAE is not associated with an improvement in discrimination.

It is possible that the distortion of the field which is the FAE is accompanied by changes in discrimination, but that it leaves the immediate vicinity of the inspection contour unaffected. Such a change would not be shown up by this experiment; and an experiment designed to measure it would be more difficult as it would need to separate changes of discrimination from changes in apparent position.

The experiment of Deutsch (1951) mentioned above shows a definite improvement in discrimination, this experiment shows none. It may be that judgements of pitch and judgements of curvature are not comparable, for instance because pitch is judged in octaves. Another possibility is that the measures used do not behave in the same way. Deutsch asked his subjects whether the tones were the same or different, thus allowing an equals category, while in the present experiment none was allowed, the intention being to measure the discrimination between curvatures rather than to measure the range over which the subjects could regard the lines as straight. It is possible that a measurement of this latter would show the expected changes; however that may be, no changes of discrimination are found in the present experiment.

The work described above has been carried out as part of the research programme of the National Physical Laboratory, and this report is published by permission of the Director of the Laboratory. The author desires to acknowledge the valuable suggestions and comments of Mr. A. Watson and of Dr. S. Papert.

REFERENCES

- DEUTSCH, J. A. (1951). A preliminary report on a new auditory after-effect. *Quart. J. exp. Psychol.*, **3**, 43.
HAMMER, E. R. (1949). Temporal factors in figural after-effects. *Amer. J. Psychol.*, **62**, 337-54.
KÖHLER, W., and WALLACH, H. (1944). Figural after-effects; an investigation of visual processes. *Proc. Amer. Phil. Soc.*, **88**, 269-357.
SAGARA, M., and OYAMA, T. (1957). Experimental studies on figural after-effects in Japan. *Psychol. Bull.*, **54**, 327.

FIGURAL AFTER-EFFECTS AND APPARENT SIZE*

BY

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Sutherland (1954) obtained results which suggest that when the retinal size of test and inspection figures is equal, the direction of the FAE may be determined by the relative apparent sizes of the two figures. Other investigators have reproduced this result when exactly the same conditions were used: when the conditions were changed the result was not obtained. In the present paper these results are discussed and an attempt is made to determine why the effect is not obtained with small variations in the experimental conditions. It is further shown that some FAE phenomena cannot be explained by the two main existing theories; these phenomena could be explained if some analysis of the stimulus is being performed before the stage of the nervous system at which the process underlying FAEs occurs. Some recent physiological evidence (Hubel and Wiesel, 1959) supports this hypothesis. If this hypothesis is correct, it is likely that further work on FAEs determined by apparent size may help to throw light on the physiological mechanisms underlying size constancy, and some further experiments are suggested.

INTRODUCTION

Three of the papers published herewith attempt to confirm some findings of mine on FAEs (Sutherland, 1954). In this experiment it was found that, when the retinal sizes of test and inspection circles were equal, the test circle appeared smaller after inspection of a figure further away, and larger after inspection of a figure nearer than itself. I interpreted the results to mean that when the retinal sizes of two figures are equal then the direction of the FAE is determined by their relative apparent sizes, i.e. a test figure will look smaller after inspection of an apparently larger figure, larger after inspection of an apparently smaller figure. I shall refer to this effect as the A-effect. Of subsequent experiments reported on this topic only two have reproduced exactly the conditions used in this experiment. Story (1961) and Day and Logan (1961) both obtained the A-effect when (i) binocular viewing conditions were used; (ii) the distances of the figures from the eye were 144 and 57.6 in.; (iii) the size of the near circle was 4 in., and of the far circle 10 in.; (iv) the circles were outline circles. Under these conditions, if the far circle is inspected, the near (test) circle appears smaller than usual; if the near circle is inspected, the far (test) circle appears larger. In five experiments (including other experimental conditions used by Story and by Day and Logan) in which conditions were not exactly the same as those I used, the A-effect was not obtained. Thus the original result appears to be reproducible under the conditions of the original experiment: it is not reproducible if these conditions are varied. This calls into question the interpretation which I originally gave. In the first half of the present paper, I shall examine what happens when the conditions of the experiment are varied and consider whether the results obtained preclude the interpretation suggested. In the second half I shall attempt to set out some theoretical considerations which affect the interpretation of these experiments and which may be of some interest in their own right. Inspection, test and comparison figures will be referred to respectively by the letters I, T and C. Table I summarizes the results obtained on this problem by different investigators using different experimental conditions.

* This paper was prepared at the request of the Editor.

APPARENT VERSUS RETINAL SIZE

Binocular vision

Using identical stimulus objects to those employed by Sutherland (1954), Story under binocular viewing conditions confirms the result I obtained, but finds that with monocular vision the T-figure falling on the same area of the retina as the I-figure tends to be seen as larger than the C, irrespective of whether the T-figure is nearer or further away than the I. She suggests that the effect found when both eyes are used may occur because under these conditions the visual angles subtended at the two eyes by the circles in each of their four possible positions are different: also since the angle between the fixation point and the centre of the circle will differ, test and inspection circles will no longer be exactly concentric. There are four reasons why this does not seem a very plausible explanation. (i) The relative displacement of the positions occupied on the retinae by different circles will be maximal for the point on each circle lying furthest from the fixation point (i.e. the outermost point

TABLE I
SUMMARY OF RESULTS ON EFFECTS OF RETINAL AND APPARENT SIZE

Experimenter	Size of circles		Distance of circles		Outline/ disc	Binocular/ monocular	Results Whether I-figure judged larger or smaller than C			
	Far	Near	Far	Near			T Near I Far	T Far I Near	Both far	Both near
Sutherland ..	10.0	4.0	144	57.6	O	B	smaller*	larger*		
McEwen ..	2.4	0.6	118	29.5	O	B	smaller*	same		
Oyama ..	3.2	1.6	118	59.0	O	M	smaller	smaller	same	smaller*
Day and Logan ..	3.2	1.6	144	72.0	O	M	smaller*	smaller*	smaller*	smaller*
	10.0	4.0	144	57.6	O	B	smaller	larger*	larger	same
					O	B	smaller	same	larger	same
Story ..	10.0	4.0	144	57.6	O	M	larger*	larger*		
					O	B	smaller*	larger*		
Terwilliger ..	4.0	2.0	96	48.0	O	B	same	same	same	same

N.B. All sizes and distances are given in inches.

* Result significant at better than the 0.05 level of confidence.

on each circle). In terms of relative displacement at the retina the effect even here is very small. I have calculated the visual angle subtended at each eye by the distance between the outermost edge of each circle and the fixation point. These calculations give the following results for the circles in the four possible positions relative to the eyes: by "same eye" is meant the eye on the same side of the head as the side occupied by a circle in relation to the fixation point. (a) Far circle, same eye: $4^{\circ} 51' 56''$. (b) Far circle, opposite eye: $4^{\circ} 51' 31''$. (c) Near circle, same eye: $4^{\circ} 52' 3''$. (d) Near circle, opposite eye: $4^{\circ} 51' 4''$. Thus it will be seen that under no condition does the amount of displacement exceed 1' of arc, and it may be doubted if this would have any effect particularly since the contours used by Story were themselves 2' 24" in breadth. Furthermore, if the distance of the near shapes from the eye varies from 57.6 in. by 0.1 in., this produces a 30" change in the visual angle subtended by the circle. It is doubtful if in any of the experiments reported the distance of the shapes from the eye was controlled to within a tenth of an inch. (ii) As Story herself states, the same effect should occur with monocular viewing provided that the alignment of the eyes and cards are the same under these conditions. When one eye alone is being used, if both the T- and I-figures are exposed on the opposite side to the eye, there will be a difference of over 20" in their visual angles: the direction of the distance will depend upon whether the I-figure is near or far, and we might expect that when it is far, the T-figure should be seen as smaller than it is; when I

is near it should be seen as larger. Unfortunately, Story does not break down her data for monocular viewing according to whether T- and I-figures were on the same or opposite sides as the eye used so that this prediction would only apply to half the trials she reports. Nevertheless, there is no sign of this trend in her results for monocular viewing. (iii) The effects to be expected due to the different spatial positions of the two eyes should be even more striking when the distance between shape and eye is less than in Story's experiment, and when the I-figure is shown to one eye and the T to the other: although these conditions have often been used in experiments on FAE no effects of this sort have been reported. (It might, however, be worth looking for them in future experiments.) (iv) Finally, although Story suggests that the different visual angles subtended by the figures at the retinae might be the explanation of the effects obtained under binocular viewing, she does not show in detail how these effects would be predicted by the geometry of the situation, and it is difficult to see how the effects found could in fact be produced in this way. Nevertheless, the suggestion is an interesting one and could be followed up by experiments in which the figures are placed closer to the eye and conditions of alternating monocular viewing are employed.

It is possible that the reason why the A-effect is obtained only when both eyes are used is that binocular vision itself provides a cue to the distance of the figures and thus to their relative apparent sizes (*v.* below): thus, the fact that the effect only occurs with binocular viewing does not necessarily conflict with the hypothesis that under some conditions the FAE may be determined by apparent size, and indeed can be interpreted within the framework of this hypothesis.

Size of circles

If *smaller* circles than those used by Sutherland are employed, the A-effect does not occur (Day and Logan, 1961; Terwilliger, 1961; McEwen 1959; Oyama, 1956): the usual result under these conditions is that the T-circle looks smaller than C whether I is nearer or further away. (It should be noted that Terwilliger did not obtain this result: when the retinal size of T and I was the same, he found no change in the apparent size of T.) This effect is also found when T and I shapes are the same distance away as one another (Day and Logan (1961), cf. also Köhler and Wallach (1944)). Day and Logan make the interesting suggestion that this shrinkage may resemble a time error effect though they do not discuss the details of how this might occur. Unfortunately, from what is known about time errors, one might expect the opposite effect with small circles. When a series of stimuli are being judged, there is usually a point in the middle of the series where (after practice) there is no constant error: above this point, time errors tend to be negative, below it, positive. We shall call this point the "adaptation point." Subjects will have an adaptation point at the start of an experiment and it will usually be shifted in the course of the experiment: now when a *small* circle is shown as I-figure this should shift the adaptation point downwards. If it shifts it downwards further for that part of the visual field on which the I-figure is shown than for other parts, we would expect the T-figure to be judged *larger* than the C-figure: the T-figure is less far away from the adaptation point at that part of the visual field than is the C-figure from the adaptation point at its part of the visual field. Day and Logan obtained exactly the opposite result to this.

Thus, there is some difficulty in applying this type of explanation, though the correspondence between the change in direction of the FAE with different sized circles (found by Day and Logan) and the change in direction of TE (found by Watson, 1957) is very suggestive. Nevertheless, Day and Logan's work does make it difficult

to interpret the A-effect as due to differences in apparent size because of their finding that when *large* circles are used and both are far away, the T-circle appears larger than the C.

Outline and filled-in circles

Day and Logan show that the A-effect occurs with outline circles but not with filled-in circles: it is hard to see what explanation could be offered for this at present.

Further discussion

One very ingenious recent experiment has demonstrated in a most convincing way that an FAE determined wholly by apparent size does occur under certain conditions: Gregory (personal communication) has shown that if the apparent size of a figure is made to shrink continuously while the retinal size remains the same, when the shrinkage in apparent size is stopped suddenly there is a dramatic increase in the apparent size of the figure. This phenomenon is very striking and is seen by all observers. Since this shows that a FAE determined by continuous change in apparent size can occur, the question arises of why it is so difficult to demonstrate the effect with static figures. There are three possible answers to this.

(1) It may be that just as with FAE due to retinal size, the effect through apparent size only occurs if the difference between the apparent sizes of the T- and I-figures is optimal (cf. the distance paradox). If this is correct, we would only expect to obtain a FAE due to apparent size under limited conditions. This suggestion could be tested experimentally by keeping one circle a constant size and distance and varying the size and distance of the other keeping retinal size equal. We would expect an effect due to apparent size to occur only within a limited range of size and distance of the other figure. In Gregory's experiment, because the apparent size of the inspection figure changes continuously, these changes are bound to straddle the point which would be optimal for producing the effect.

(2) The conditions of the experiments performed with static figures are such that there may be a temptation to judge in terms of retinal size: it is known that when two shapes of different real size are aligned side by side, subjects tend to make judgements in terms of retinal size (Joynson and Kirk, 1960). It would be interesting to test for the occurrence of the A-effect, using for T- and C-figures two shapes of the same physical size but different retinal sizes at different distances away from the observer and not aligned opposite one another. The T-circle could be kept the same retinal size as the I, and the C-circle would be a different retinal size: subjects would be asked to compare the *real* size of T- and C-figures. These experimental conditions should tend to favour judgements in terms of apparent physical size rather than apparent retinal size.

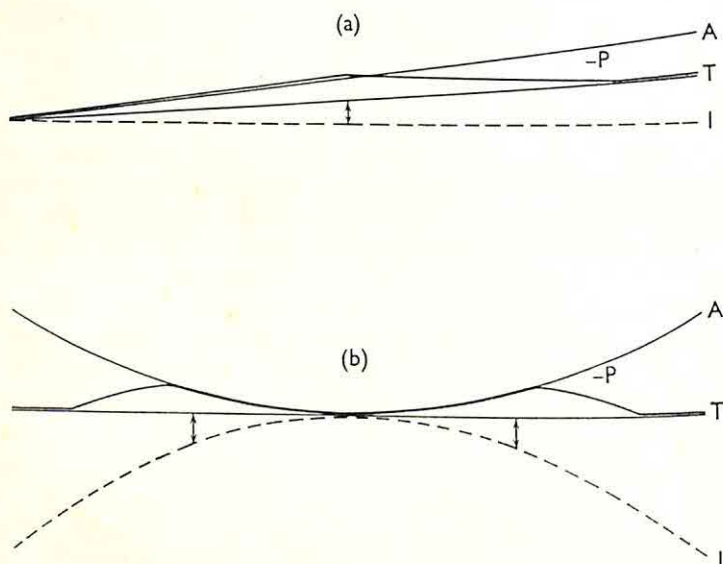
(3) It may be that apparent size only influences FAE when the apparent size has changed continuously, i.e. where there has been an apparent movement effect: if established this would be an important finding since it would reveal a difference in the mechanisms underlying apparent movement and judgements of apparent size (*v.* below). This could only be established by a thorough investigation of the static A-effect along the lines set out in (1) and (2) above.

THEORETICAL CONSIDERATIONS

The work of Hubel and Wiesel (1959) suggests a new theoretical approach to FAE problems. In order to see the experiments described above in perspective, it may be worth setting out briefly what this approach is: it has suggested itself independently

to a number of workers in the field, and Papert is currently engaged on testing some of its implications. It must be stressed that a new approach is necessary since the sort of theory espoused by Köhler and Wallach (1944) and by Osgood and Heyer (1952) is unable to account for many of the phenomena of FAE. They both assume that inspection of a contour results in any contour subsequently falling near the second contour being seen as displaced away from it: the amount it is displaced is said to depend upon the distance separating the two contours on the retina, and there will be a point at which displacement is maximal. Three instances of well attested phenomena which this theory is unable to explain will be quoted. (1) In Figure 1, if the I-line is fixated, the T-line should appear as shown (P): displacement should be small where I and T lie near together gradually increasing to a maximum and then decreasing. In fact T is seen occupying the position of line A. (2) Similarly

FIGURE 1



when a curved line is shown, and a straight line used as I-figure, the straight line should appear like line P in Figure 1 (b) but in fact appears like line A. (3) The theories are unable to account for the after effect of seen motion. Both theories under discussion assume that the FAE occurs before any analysis of the stimuli is undertaken.

Hubel and Wiesel have demonstrated by recording from single cells that in the cat considerable analysis of the stimulus on the retina occurs at or before the level of the striate cortex. In particular they present evidence to show that in the striate cortex there are cells whose response is determined by the orientation of lines on a given part of the retina; i.e. the orientation of lines is coded in separate fibres at this level of the cat visual system. If we assume that there are cells with similar receptive fields in human beings we have a very simple explanation of the effect shown in Figure 1 (a): inspection of a line in one orientation will result in heavy firing of the cells maximally responsive to lines in this orientation, and to some firing of cells maximally responsive to lines in neighbouring orientations. If any adaptation occurs in these cells as a result of prolonged firing, when a T-contour in a slightly different orientation to the I line is exposed on the same part of the retina, the cells fired maximally by it will be ones which are normally maximally responsive to

contours in orientations lying further away from the orientation of the I-figure. It is reasonable to suppose that the orientation in which a contour is seen will depend upon the balance of firing in cells representing contour orientation: the firing in any one cell will be determined partly by the contrast of the contour with its background, etc., but such effects would be balanced out if the ratio of firing in all cells sensitive to orientation in a given region of the retina were computed. If there are also cells sensitive to curvature of a line a similar mechanism would explain the sort of finding depicted in Figure 1 (*b*). As yet there is no physiological demonstration of the existence of such cells.

Hubel and Wiesel have, however, found cells which respond differentially according to the direction in which a stimulus is moved across the retina. If direction of movement is coded in single cells in human beings, adaptation in these cells might clearly underly the after-effect of movement. Once again the direction in which something is seen to move might depend upon the ratios of firing in cells sensitive to movement in different directions, and after prolonged movement in one direction a stationary image would produce less firing in the cells which had just been stimulated than normally, hence apparent movement in the opposite direction would be seen to occur.

This explanation of FAE is based on sound physiological evidence and is so simple that it seems highly convincing. It does not, however, explain mere displacements in apparent spatial position occurring as a FAE: for this phenomenon, the Osgood and Heyer type of explanation appears reasonably plausible. This explanation in fact fits well with the explanation outlined above since Osgood and Heyer argue that the position at which a contour is seen itself depends upon ratios of firing in different cells. The possibility of explaining the FAEs produced under different conditions in terms of simple physiological analysing mechanisms in the visual system increases the interest of further work on FAEs: if we can determine the exact conditions under which FAE's occur, this knowledge should help us to specify more accurately the nature of the underlying physiological analysing mechanisms.

The determination of the conditions under which effects due to apparent size occur becomes a particularly interesting problem. It is possible that some apparent size and apparent movement effects occur because central mechanisms, through efferents in the visual pathways, put a bias on the cells performing analysis of the stimulus between receptors and cortex. For instance, if the size of an object is itself coded in single cells somewhere between receptor and visual cortex, the cells stimulated by an object of given retinal size might vary according to efferent firing produced by a mechanism which analysed the distance away of the object. Possibly some factors determining the apparent size of an object operate at this level and others at a higher level. Thus the depth effect due to stereoscopic vision (which seems to have an innate basis, Ogle (1950)), might operate at this level and other depth effects at a higher level in the nervous system: this would explain why Story obtained the A-effect binocularly but not monocularly. Again if, after further investigation, it is found that no A-effect takes place with static figures although it does with figures whose apparent size changes continuously, this would suggest that apparent movement effects may be determined at a different level of the nervous system from apparent size effects. It is, of course, always possible that even where a factor determining apparent size operates at a higher level of the nervous system, it will operate in such a way as to result in FAEs: nevertheless, if it can be shown that FAEs due to apparent size occur under some conditions and not under others this suggests fundamental differences in the mechanisms at work in different conditions, and gives a promising lead towards working out what is the neurological basis of such phenomena as size constancy—a question about which at the moment almost nothing is known.

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REFERENCES

- DAY, R. H., and LOGAN, J. A. (1961). A further investigation of apparent size and retinal size as determinants of the figural after-effect. *Quart. J. exp. Psychol.*, **13**, 193-203.
- HUBEL, D. H., and WIESEL, T. N. (1959). Receptive fields of single neurones in the cat's striate cortex. *J. Physiol.*, **148**, 574-91.
- JOYNSON, R. B., and KIRK, N. S. (1960). An experimental synthesis of the Associationist and Gestalt accounts of the perception of size. Part III. *Quart. J. exp. Psychol.*, **12**, 221-30.
- KÖHLER, W., and WALLACH, H. (1944). Figural after-effects: an investigation of visual processes. *Proc. Amer. phil. Soc.*, **88**, 269-357.
- McEWEN, P. (1959). Figural after-effects, retinal size and apparent size, *Brit. J. Psychol.*, **50**, 41-7.
- OGLE, K. N. (1950). *Researches in Binocular Vision*. London.
- OSGOOD, C. E., and HEYER, A. W. (Jun.) (1952). A new interpretation of figural after-effects. *Psychol. Rev.*, **59**, 98-118.
- OYAMA, T. (1956). Temporal and spatial factors in figural after-effects. *Jap. psychol. Res.*, **3**, 25-36.
- STORY, A. W. (1961). Has apparent size been tested as a factor in figural after-effects? *Quart. J. exp. Psychol.*, **13**, 204-8.
- SUTHERLAND, N. S. (1954). Figural after-effects, retinal size and apparent size. *Quart. J. exp. Psychol.*, **6**, 35-44.
- TERWILLIGER, R. F. (1961). Retinal size as a determiner of the direction of size distortion in figural after-effects. *Quart. J. exp. Psychol.*, **13**, 209-17.
- WATSON, W. A. (1957). Contrast assimilation, and the effect of central tendency. *Amer. J. Psychol.*, **70**, 560-8.

SHORTER ARTICLES AND NOTES

CHANGES IN THE VISUAL THRESHOLD AS A FUNCTION OF SUBCEPTION

BY

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In a previous paper (Dixon, 1958b) one of the authors reported an experiment which suggested that apparent changes in the threshold for one eye occur as a function of the emotionality of stimulus material presented below threshold to the other eye. The following experiment describes an attempt to investigate further the validity of this conclusion. The results are consistent with those from the previous research.

Many of the experiments purporting to demonstrate perceptual defence have been criticized on two counts, first that subjects voluntarily suppress report of anxiety producing stimulus material, secondly that any differences in threshold that do occur may be ascribed to differences in familiarity between the different stimulus items.

In an attempt to resolve this controversy, discussed in a previous paper (Dixon, 1958b), one of the authors approached the problem in a rather different way. Instead of trying to obtain recognition thresholds for different sorts of stimulus material, the awareness threshold for a spot of light presented to one eye was continuously recorded during the presentation below threshold to the other eye of different sorts of stimulus material. Using the method of "closed loop" control (Oldfield, 1955) whereby the subject controls intensity of the test patch, this study provided some support for the perceptual defence hypothesis. Thresholds for the left eye were found to be significantly higher when sexual and tabu material was presented to the right eye than when neutral words were used. While this finding could not be explained in terms of voluntary suppression (since the subject was never aware of the stimulus nor required to make a verbal response) the possibility was not ruled out that the observed differences in threshold were a function of differences in familiarity of the stimulus items.

The experiment† reported here attempts to deal with these and other possible criticisms by carrying the investigation of apparent threshold changes one stage further. In particular, it is concerned with testing the following hypotheses:—

- (1) Familiarity of the stimulus material is not a decisive factor in determining the threshold.
- (2) Anxiety engendered by a given word will evoke higher thresholds during the presentation, subsequently, of other words.
- (3) Thresholds will correlate positively with subsequent consciously made assessments of the anxiety provoking power of the words used.
- (4) It is the meaning of the stimulus words and not the quantity of light which they transmit that determines threshold changes.

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† Carried out within the context of a larger study (Haider and Dixon, 1961) to investigate the effect of training and fatigue on the visual threshold.

Apparatus

The apparatus was that used in the previous experiment and described fully in an earlier paper (Dixon, 1958a).

Subjects

The ten subjects, of whom four were female, were M.R.C. and secretarial personnel from the Department of Psychology, University College.

Method of procedure

As in the previous experiment the method involved taking a continuous measure of the visual threshold for one eye while presenting different items of stimulus material at subthreshold intensities to the other eye.

The stimulus material consisted of the following four words:—

Stimulus words

CANCER
BREAST
RECANT
STANCE

All four words were equated for brightness, length and as far as possible for letter content. They were tentatively chosen as being likely to show differences in emotional loading on the subsequent assessment.

During each trial 21 threshold readings were taken at 30-sec. intervals. (These readings represent the area under the threshold curve as read off from an integrating counter geared to the wedge controlling light intensity.) The four stimulus items were presented once each for 30 sec. but the interval between these stimulus presentations was varied (in number of 30-sec. blocks) from trial to trial and from subject to subject. For subjects in Group A the order of presentation of the stimulus words was "cancer," "recant," "breast," "stance," for Group B it was "stance," "breast," "recant," "cancer."

Each subject had two trials in which the four stimulus words were presented and two trials in which the four stimulus presentations involved projecting a rectangle of light of the same intensity and size as the words. For each subject the two trials involving words were done within 12 hr. while those involving the simple light stimulus were carried out three weeks later. This somewhat asymmetrical design was introduced to obviate any possibility of subjects expecting the introduction of stimulus words. It was assumed moreover that a three week gap between the two parts of the experiment would reduce to a minimum any chance of carry over from the one part to the other.

Subsequently 56 subjects who had not taken part in the experiment were asked to rank the four stimulus words in order of the degree to which they were productive of unpleasant emotion. (To reduce the possibility of experimenter influence this part of the experiment was carried out by a different experimenter.)

RESULTS

The frequency hypothesis

The following table shows the frequency scores for the four words, from the Thorndike Lorge List, and the mean sensitivity changes recorded during their presentation.

TABLE I

<i>Stimulus word</i>	<i>Frequency</i>	<i>Mean changes in threshold*</i>	
		<i>Trial 1</i>	<i>Trial 2</i>
CANCER	5 per million	4.20	2.40
BREAST	50-100 per million	0.35	5.35
RECANT	1 per million	-1.15	2.1
STANCE	1 per million	0	2.65

* Integrator score differences between stimulus and adjacent no stimulus presentations.

These results support the view that there is little relationship between the familiarity of a word and the threshold recorded during its presentation. If anything they go the opposite way from that found in the previous experiment (Dixon, 1958*b*) wherein the highest thresholds coincided with the least familiar stimulus words.

The effect of verbal as opposed to "light" subliminal stimuli

A possible criticism of the findings in the previous research was that the observed differences in threshold could be attributed to the differing amounts of light transmitted by the different stimulus items. The results in the present experiment showed that, whereas subliminal verbal stimuli evoked a significant rise in threshold, light stimuli were accompanied by an insignificant lowering of the threshold.

For the purpose of comparison the thresholds recorded during subliminal verbal and light stimulation respectively were compared with the thresholds recorded immediately prior to and succeeding each such presentation. The individual results are shown in the following table:—

TABLE II
MEAN THRESHOLDS*

Subject	Subliminal "words" stimulus		No stimulus before or after		Subliminal "light" stimulus		No stimulus before or after	
	Trial 1	Trial 2	Trial 1	Trial 2	Trial 1	Trial 2	Trial 1	Trial 2
1	45.8	47.0	47.5	49.8	52.0	45.5	49.6	40.6
2	33.8	16.0	37.5	18.0	54.2	71.0	56.1	57.4
3	58.3	48.7	58.9	54.8	83.3	77.7	79.2	72.9
4	24.5	16.3	24.0	18.4	37.8	30.0	36.1	34.5
5	31.8	31.0	28.8	31.1	53.0	46.3	51.4	42.6
6	47.0	47.5	46.5	48.3	70.0	63.3	66.1	54.5
7	56.5	52.8	59.4	56.6	57.8	56.0	59.4	60.4
8	53.3	40.5	57.0	43.0	66.3	57.0	66.6	56.9
9	50.5	45.8	51.4	51.8	43.3	58.3	50.0	60.3
10	31.5	33.0	30.5	38.3	28.5	30.3	28.4	31.4
Means	43.30	37.86	44.15	41.01	54.62	53.54	54.29	51.15

* Integrator scores.

Results for the group are summarized in terms of threshold differences in Table III.

TABLE III

	Threshold differences between							
	Subliminal verbal presentations and "before" and "after" no stimulus presentations				Subliminal light presentation and "before" and "after" no stimulus presentations			
	Mean change in threshold	No. of subjects showing rise in threshold	<i>t</i>	<i>p</i>	Mean change in threshold	No. of subjects showing fall in threshold	<i>t</i>	<i>p</i>
1st run	0.85	6	1.23	>0.05	-0.33	6	—	>0.05
2nd run	3.15	10	4.74	<0.002	-2.39	6	1.29	>0.05
Both runs	2.00	9	3.80	<0.005	-1.36	5	1.15	>0.05

To confirm these findings thresholds recorded during subliminal stimulation were also compared with the mean thresholds for all "no stimulus" presentation.

TABLE IV
MEAN THRESHOLDS*

Subject	Subliminal "words" stimulus		All no stimulus periods		Subliminal "light" stimulus		All no stimulus periods	
	Trial 1	Trial 2	Trial 1	Trial 2	Trial 1	Trial 2	Trial 1	Trial 2
1	45.8	47.0	47.1	50.1	52.0	45.5	51.4	41.6
2	33.8	16.0	41.0	18.2	54.2	71.0	56.9	57.5
3	58.3	48.7	62.2	56.4	83.3	77.7	78.5	74.5
4	24.5	16.3	23.2	16.1	37.8	30.0	36.4	31.2
5	31.8	31.0	28.4	30.7	53.0	46.3	54.9	48.3
6	47.0	47.5	47.4	46.7	70.0	63.3	64.0	57.4
7	56.5	52.8	59.0	56.6	57.8	56.0	58.2	58.2
8	53.3	40.5	55.4	41.7	66.3	57.0	64.0	58.4
9	50.5	45.8	53.1	51.8	43.3	58.3	53.2	62.2
10	31.5	33.0	31.7	43.0	28.5	30.3	29.0	34.0
Means	43.30	37.86	44.85	41.13	54.62	53.54	54.65	52.33

* Integrator scores.

Results for the group are summarized in terms of threshold differences in Table V.

TABLE V

	Threshold differences between							
	Subliminal verbal presentations and mean of all "no stimulus" presentations				Subliminal light presentation and mean of all "no stimulus" presentations			
	Mean change in threshold	No. of subjects showing rise in threshold	<i>t</i>	<i>p</i>	Mean change in threshold	No. of subjects showing fall in threshold	<i>t</i>	<i>p</i>
1st trial	1.55	8	1.69	>0.05	0.03	5	—	>0.05
2nd trial	3.27	7	2.84	<0.02	-1.21	4	—	>0.05
Both trials	2.41	7	2.86	<0.02	-0.59	6	—	>0.05

Whereas the verbal stimuli evoked significantly *higher* thresholds relative to the before and after no stimulus periods in the second trial (mean = 3.15) than in the first (mean = 0.85) the light stimuli evoked *lower* thresholds in the second trial (mean = -2.39) than they did in the first (mean = -0.33). For the verbal stimuli this difference between trials was significant at the 5 per cent. level of confidence ($t = 2.65$). Eight subjects contributed to this effect. For the light stimuli the difference fell short of that required for statistical significance ($t = 1.120$).

While not significant in the first trial the difference between verbal and light stimuli in their effect upon the threshold during the second trial (mean = 5.54) was significant ($t = 3.21$, $p < 0.01$). Eight subjects contributed to this effect.

Taking both runs together the difference between words and lights was significant at the 2 per cent. level of confidence ($t = 2.95$). Eight subjects showed this effect. Finally the difference between words and lights in the first run was significantly less than in the second run ($t = 2.585$, $p < 0.05$). Seven subjects showed this effect.

TABLE VI
DIFFERENCES* BETWEEN "WORDS" AND "LIGHT" IN THEIR EFFECT
UPON THE THRESHOLD

Subject	Trial 1	Trial 2
1	4.1	7.7
2	1.8	15.6
3	4.7	10.9
4	1.2	-2.4
5	-1.4	3.8
6	3.4	9.6
7	1.3	-0.6
8	3.4	2.6
9	-5.8	4.0
10	-0.9	4.2
Means	1.18	5.54

* Words raised the threshold, "light" tended to lower it. The differences are between integrator raw scores during the two sorts of stimulation and the respective "before" and "after" no stimulus periods.

Differences between the verbal stimuli

Table VII shows mean differences in threshold change for the four stimulus words. In their apparent effect upon the visual threshold the stimulus words differed from each other very much more during the first run than they did during the second run.

In the first run, and for eight subjects, only the word "cancer" evoked thresholds significantly higher ($t = 2.94$, $p < 0.02$) than those recorded during the adjacent "no stimulus" presentations:—

TABLE VII
MEAN CHANGES* IN THRESHOLD UNDER DIFFERING SUBLIMINAL STIMULUS
CONDITIONS

	Cancer	Breast	Recant	Stance
1st trial	4.20	0.35	-1.15	0
2nd trial	2.40	5.35	2.1	2.65

* Differences between integrator scores during subliminal presentation of words and adjacent no stimulus periods.

In Trial I the difference between the word "cancer" and the two words predicted as being relatively neutral, "recant" and "stance," was significant at the 2 per cent. level of probability ($t = 2.76$). Eight subjects contributed to this effect. In the second run there were no significant differences between the stimulus words.

Independent ranking of words

Table VIII shows the mean rankings obtained for the four words from the 56 subjects who subsequently ranked them for the degree to which they are "productive of unpleasant emotion."

TABLE VIII

	<i>Cancer</i>	<i>Recant</i>	<i>Breast</i>	<i>Stance</i>
Mean rank	1.2	2.5	2.9	3.4

Coefficient of concordance: $W = 0.56$ ($p < 0.01$)

This result is consistent with the fact that "cancer" evoked the largest and only significant rise in threshold.

Changes in the motor performance

Anticipating the possibility that the apparent threshold changes were in fact due to changes on the motor side, the number of control movements made by subjects and the total length of the threshold curve were compared between the different stimulus conditions. As a result of these comparisons no significant differences were found.

DISCUSSION

The results support the conclusion from an earlier research (Dixon, 1958b) that anxiety provoking stimuli presented below threshold to one eye will evoke a rise in the visual threshold as measured for the other eye. They indicate moreover that the relative familiarity of the stimulus items does not constitute a decisive factor. In the previous experiment those stimulus items which appeared to evoke higher thresholds were less familiar than the other and neutrally toned items. In this experiment the reverse held good.

A further point of significance regarding the present experiment is that it showed a relationship between conscious judgements regarding the emotionality of the stimulus words and the effect of these same words upon the threshold.

Finally a comparison of the trials suggests the possibility that anxiety engendered by a particular word can generalize to other hitherto neutral words when these are presented subsequently. Apropos of this it is interesting that "breast," a frequent site of cancer, was the one word which evoked the highest threshold in the second trial.

As to an explanation for these phenomena there would seem to be at least three possibilities. The first might be that "anxiety," engendered following unconscious discrimination of threatening material, reduces peripheral sensitivity, either through efferents to the retina or through the effect of an adrenergic response upon local synaptic transmission. A second explanation might be in terms of some more centrally placed filtering system which, when blocking information of a threatening nature, also impedes the passage of other simultaneous transmissions. Yet a third explanation might be in terms of a change in the criteria for decisions based upon recognitive experiences. The latter effect might possibly result from a general decline in cortical activity through temporary inhibition of the arousal system. These possibilities are discussed in another paper (Dixon, 1960).

REFERENCES

- DIXON, N. F. (1958a). Apparatus for continuous recording of the visual threshold by the method of "closed loop" control. *Quart. J. exp. Psychol.*, **10**, 62-3.
 DIXON, N. F. (1958b). Apparent changes in the visual threshold as a function of subliminal stimulation. *Quart. J. exp. Psychol.*, **10**, 211-19.

- DIXON, N. F. (1960). Apparent changes in the visual threshold—central or peripheral? *Brit J. Psychol.*, **51**, 297-310.
- HAIDER, M., and DIXON, N. F. (1961). Influences of training and fatigue on the continuous recording of a visual differential threshold. *Brit. J. Psychol.*, **52**, 227-37.
- OLDFIELD, R. C. (1955). Apparent fluctuations of a sensory threshold. *Quart. J. exp. Psychol.*, **7**, 101-15.

THE RELATIVE INTELLIGIBILITY THRESHOLD FOR ONE'S OWN NAME COMPARED WITH OTHER NAMES

BY

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Two recent experiments (Moray, 1959; Oswald, Taylor and Treisman, 1960) have shown that in dichotic listening, and in sleep one's own name seems to be responded to selectively. We have shown for 10 subjects, each listening binaurally to the same recording of a list of their names repeated at random 10 times, and masked by noise, that the subject's own name had a significantly lower threshold than other names. The effect we found was not significantly different from the one found by Oswald *et al.* during sleep and by Moray for the dichotic situation.

These three experiments taken together suggest that the same pattern-analysing mechanism is involved in normal listening, dichotic listening to the rejected message, and in discrimination during sleep.

INTRODUCTION

In a recent paper Moray (1959) has confirmed Cherry's (1953) finding that when subjects are "shadowing," i.e. speaking aloud with a delay of about 1 sec., a continuous message presented to one ear alone, they are almost completely unaware of the content of a message given simultaneously to the other ear. Moray has, however, extended Cherry's observations on the amount of information which the subject receives from the rejected message. Subjects are aware of the presence of a message in the other ear, whether it is speech or some other sound, whether it is a male or a female voice. Most important, however, is Moray's finding that subjects will hear messages presented to the other ear provided the message is preceded by the subject's own name. All the other observations suggest merely that the message to the rejected ear has been considerably attenuated so that only a very limited amount of information can be obtained from it. The effect of the subject's own name seems to Moray to require a concept additional to that of attenuation, since it appears to be so selective. Moray suggests that the "block in dichotic shadowing occurs at quite a high level, and that the block is central to some pattern-analysing mechanism." This idea of a pattern-analysing mechanism, situated relatively peripherally in the nervous system, is an extremely attractive one which could be used to explain many of the phenomena of subliminal perception, perceptual defence and selective habituation. There also seems to be some physiological evidence for such an idea from work such as that of Sharpless and Jasper (1956) and Culler, Coakley, Shurrager and Ades (1939). Nevertheless, it seemed to us that Moray's experiment did not provide evidence for such a mechanism in the dichotic listening situation, since he gives no data for the relative intelligibility of one's own name under normal listening conditions. If one wished to demonstrate a mechanism more selective than a simple attenuator one would have to show that in the dichotic situation there was a bigger difference in the intelligibility of one's own name compared with other messages than there is in the ordinary listening situation. The present experiment sets out to provide the necessary control. Our data coupled with Moray's data constitute a test of the hypothesis whereas Moray's data alone did not.

METHOD

Apparatus

A Brenell Mark V twin channel tape recorder was used to provide the stimuli. On one channel the numbers 1 to 100 were recorded with a 5-sec. interval between each

number. These numbers were used to assist the recording of the experimental material on the other channel which consisted of 10 christian names, five male, five female, each recorded 10 times in random order. This channel was played binaurally to the subject via an attenuator and a pair of Brown type K earphones. A 50 cycles per sec. masking tone was provided from an Advance oscillator and played to the subject through a pair of miniature hearing aid earphones worn underneath the headphones. The numbers recorded on the first channel of the tape recorder were played back to the subject through a loudspeaker to indicate the spacing of the names. The separate transducers were used in each case to overcome mixing difficulties.

Procedure

Each subject's threshold was determined approximately with a trial tape. The experimental tape was then played at an intensity expected to give about 50 per cent. intelligibility for the names. In many cases no names at all were heard at the chosen level so after the first 20 or so names the tape was started again at a higher intensity. Each subject was presented with a proforma with 100 spaces in which he or she was told to write down what they heard. They were instructed as follows:

"This is a test to ascertain the acuteness of your hearing. I am going to play to you a record which contains the numbers 1 to 100 at 5-sec. intervals, these numbers correspond to the numbers on your score sheet. You will always hear the numbers at the same loudness from the loudspeaker. After each number there will be a word from the earphones which at first you may not be able to hear. When you are able to recognize a word I want you to write it down, after the number it follows, on the score sheet."

Our subjects each had different names and each of the names on the tape was the name of one of our subjects. By this design we hope we have eliminated the effect of the greater intelligibility of some names.

RESULTS

The results are shown in Tables I and II. When all the subjects' results are pooled, as in Table II, it can be seen that in the present experiment the subject's own name was recognized on 77 per cent. of the occasions it was presented, while other names were recognized only on 50.5 per cent. of occasions. Table II also contains for comparison Moray's data for the dichotic situation and some data obtained by Oswald, Taylor and Treisman (1960) in an experiment on sleep which will be discussed later. Mere inspection reveals how similar are the results of these three experiments.

Statistical treatment of data. A simple χ^2 test shows that the differences between the intelligibility of one's own name and of other messages is significant in all three experiments. It is more difficult to compare the size of the effect in the different experiments. One must make allowance for the fact that only a small difference in probability of response would have been found if both were high (i.e. near 100 per cent.) or low (i.e. near 0 per cent.). To take this factor into account we have converted the probabilities to probits and also obtained an estimate of the standard error of the probit by subtracting its standard error from the higher probability and converting the resulting probability to a probit value; the difference between this and the probit of the higher probability we have taken as an estimate of the standard error in standardized (probit) units. Similarly we added its standard error to the lower probability and obtained an estimate of the standard error in probit units for that also. We then calculated t as follows:

$$t = \frac{(\text{difference in probits expt. 1}) - (\text{difference in probits expt. 2})}{\sqrt{(\text{higher SE probits expt. 1})^2 + (\text{lower SE probits expt. 1})^2 + (\text{higher SE probits expt. 2})^2 + (\text{lower SE probits expt. 2})^2}}$$

The only important assumption involved in this calculation is the assumption that the variability of the threshold in the two experiments will be equal so that the standardization is made onto the same scale.

If this method of comparing the differences in probability for hearing one's own name compared with other messages is accepted, we find that in none of the three experiments is there any difference in the selectivity of the system for the subject's own name compared with other messages.

TABLE I

Subjects	Names	Mike	John	Alan	Brian	Roy	Helen	Eva	Adelaide	Judy	Janice	Total other names	Total own name
Mike	..	10	10	0	7	10	7	2	8	4	0	48	10
John	..	9	9	2	10	4	4	0	0	7	0	36	9
Alan	..	6	9	7	8	0	4	1	10	1	0	39	7
Brian	..	9	9	0	10	0	0	1	0	4	0	23	10
Roy	..	10	9	10	7	10	10	0	9	9	0	64	10
Helen	..	10	9	0	10	9	3	5	9	0	2	54	3
Eva	..	10	10	2	10	0	7	6	0	3	0	42	6
Adelaide ..	..	2	7	2	4	8	10	5	10	4	0	42	10
Judy	..	10	6	1	8	2	9	4	9	6	0	49	6
Janice	..	7	9	2	9	10	8	0	4	9	6	58	6
Total	..	83	87	26	83	53	62	24	59	47	8	455	77

Proportion of "own names" heard = $77/100 = 0.770$.

Proportion of "other names" heard = $455/900 = 0.505$.

Table I

Shows the number of times each subject heard correctly each of the names. False responses were negligible and no correction has been made for guessing.

TABLE II

	A	B	C
	<i>Present experiment</i>	<i>Dichotic situation (Moray)</i>	<i>Sleep situation (Oswald et al.)</i>
Own name	77/100	20/39	33/131
Other message	455/900	4/36	15/124
<i>p</i> difference	<0.001	<0.01	<0.01

t tests for the significance of the difference in the size of the effect give:

for A and B $t = 0.87$ $p > 0.3$

for A and C $t = 0.95$ $p > 0.3$

Table II

Shows the combined data for the present experiment and the experiments of Moray (1959) and Oswald *et al.* (1960) together with the significance levels of the differences within each experiment and between the present experiment and the other two. See text for account of statistical treatment used.

DISCUSSION

It may be thought that our experiment does not provide an adequate control for Moray's experiment, since he compared the intelligibility of messages such as "change to the other ear" with the intelligibility of the same messages preceded by the subject's own name. However, we felt that it was more important to produce a balanced design which would make allowance for the greater intelligibility of some names than others. It could be argued that Moray has failed to show any effect specific to the subject's name since he did not test the effect of a particular name on subjects whose name it was not. Nevertheless, in spite of the inadequacies of Moray's experimental design we believe his result is a genuine one since, as can be seen in Table II, his results are very similar to those in the present experiment and in the experiment of Oswald *et al.*

Oswald *et al.* who did not postulate a secondary pattern-analysing mechanism to account for their results, used an experimental design very similar to our own, except that their subjects were sleep deprived, so that they fell asleep during the experiment. The subjects were instructed to respond by making a hand movement when they heard their own name and also when they heard one other name; the figures we have used for comparison with our own, relate only to the probability of response to the subject's own name and the other name to which they were told to respond. Oswald *et al.* also looked for K-complexes in their subject's e.e.g. records after each name. They found 73/110 following the subject's own name and 48/109 following the other name. These probabilities are almost identical with the ones we obtained. There is, therefore, a very impressive amount of agreement among these three very different experiments concerning the relative intelligibility of one's own name. It seems an obvious conclusion to suppose that the same pattern-analysing mechanism has been involved in each case and that no subsidiary pattern-analysing mechanism is required to account for behaviour during dichotic listening or during sleep. Although there seems to be a great deal of physiological evidence that the nervous system is organized hierarchically, with some duplication of function at the different levels, none of the experiments we have considered provides evidence for a functional hierarchy with more than one pattern-analysing mechanism.

We wish to thank Mr. A. B. Royse for his advice on the statistical treatment of the data.

REFERENCES

- CHERRY, E. C. (1953). Experiments on the recognition of speech with one and two ears. *J. acoust. Soc. Amer.*, **25**, 975.
- CULLER, E. A., COAKLEY, J. D., SHURRAGER, P. S., and ADES, H. W. (1939). Differential effects of curare upon higher and lower levels of the central nervous system. *Amer. J. Psychol.*, **52**, 266-73.
- MORAY, N. (1959). Attention in dichotic listening: affective cues and the influence of instructions. *Quart. J. exp. Psychol.*, **11**, 56-60.
- OSWALD, I., TAYLOR, A., and TREISMAN, M. (1960). Discrimination responses to stimulation during human sleep. *Brain*, **83**, 440.
- SHARPLESS, S., and JASPER, H. (1956). Habituation of arousal reaction. *Brain*, **79**, 655.

CENTRAL INTERMITTENCY: SOME FURTHER OBSERVATIONS*

BY

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When the human subject is asked to respond to two stimuli separated by a very short time interval the response to the second stimulus may be relatively delayed. The present experiment is designed to test the hypothesis that the elements of the delay attributable to the sensory and motor components of the stimulus-response situation may be subserved by relatively independent neural mechanisms. The results obtained are compatible with this hypothesis and are briefly discussed.

INTRODUCTION

Several theories have been advanced to explain the phenomena of the Psychological Refractory Period (e.g. Poulton, 1950; Welford, 1952; Davis, 1956). The present experiment was designed not to test any specific hypothesis but to investigate the possibility that the delay often observed in the responses to the second of two closely spaced stimuli might be attributable to different aspects of the task each dealt with by relatively independent neural mechanisms. In particular the experiment was designed to distinguish that element of the delay which might be attributed to the processing of the information in the first stimulus from that attributable to the organization and execution of the motor response.

EXPERIMENTAL PROCEDURE

The recording apparatus and the display were those used in experiments already reported (e.g. Halliday, Kerr and Elithorn, 1960). The subject was seated facing a black bakelite board with a central fixation point which could be illuminated as a warning signal, to the left and right of which were the stimulus signals consisting of two clusters of five neon indicator bulbs. He responded by pressing one or both of two reaction time keys corresponding to the sides on which the visual stimuli appeared.

In the first part of the experiment, pairs of stimuli were presented at 20 time intervals, under two conditions. The inter-stimulus intervals ranged from 0 to 760 millisecc. by 40 millisecc. stages. The conditions were as follows:—

The first condition (subsequently referred to as condition *a*) was a two-stimulus one of the disjunctive type; 750 millisecc. after a warning the first stimulus (*S*₁) was presented on either the left or right followed by a second stimulus (*S*₂) on the opposite side at one of the above time intervals. Each stimulus consisted of the illumination of all five neons and the subject responded by pressing the appropriate key.

Under the second condition (condition *b*) the first stimulus *S*₁ was again presented on either the left or right but the second stimulus was presented on the same side, and the subject was instructed to respond only to the second stimulus. *S*₁ consisted of the illumination of one neon, *S*₂ consisted of the illumination of the remaining four. The subject was told that the first stimulus indicated on which side *S*₂ would be presented.

Blocks of 40 stimulus-response situations were presented in a predetermined random order. Twenty-five blocks under condition *a* and 25 under condition *b* were presented in the order *abba*, *baab*, . . . the subject being told which condition to expect.

Following the completion of the main experiment, single disjunctive reaction times were obtained as an additional control (condition *c*). On these occasions the situation was the same as under condition *a* except that the first stimulus at time 0 was omitted, only one response being required. Blocks of 40 situations were replicated 13 times.

* This paper is based on a communication to the Experimental Psychology Group on 10th July, 1957.

RESULTS

Practice effects were marked during the first part of this experiment and in order to have a consistent estimate of practised performance it was decided to analyse separately the last 10 responses for each stimulus situation for each hand. As concordance between the two hands was high, the observations have been combined. The results are shown in Table I and Figures 1 and 2.

TABLE I
MEAN REACTION TIMES FOR THE LAST 10* RESPONSES TO S2 FOR BOTH
HANDS. SUBJECT C.L.

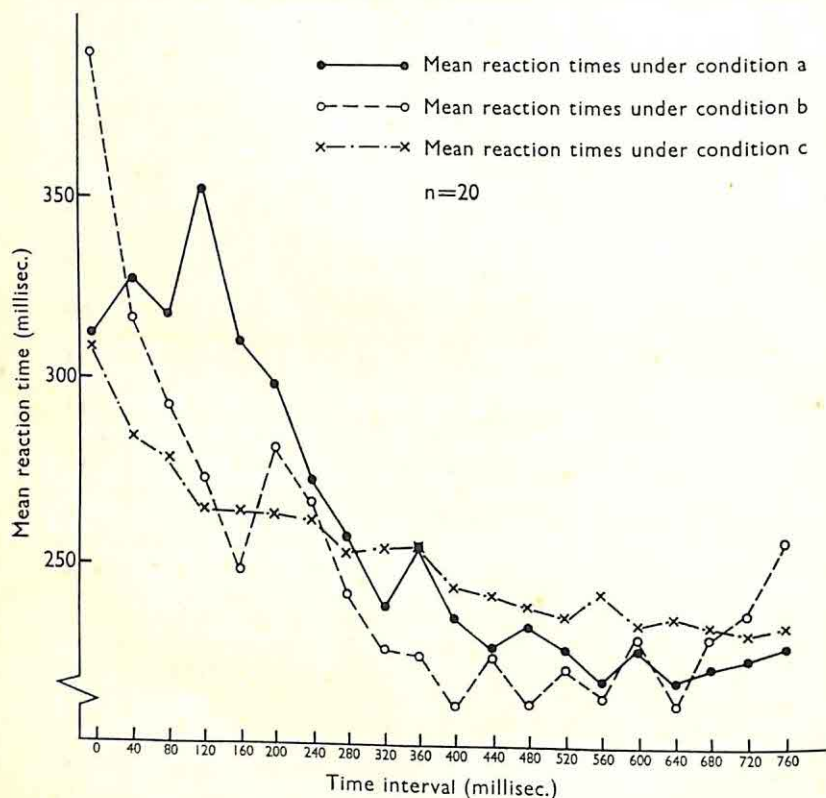
<i>Interval</i>	<i>Condition a</i>	<i>Condition b</i>	<i>a-b</i>	<i>Condition c</i>	<i>a-c</i>	<i>b-c</i>
<i>Millisec.</i>	(S.E.)	(S.E.)		(S.E.)		
0	312 (6.8)	392 (12.9)	-80	299 (6.6)	13	93
40	329 (7.9)	317 (6.3)	12	277 (5.9)	52	40
80	319 (8.1)	294 (6.1)	25	271 (4.8)	48	23
120	354 (9.8)	265 (6.2)	89	268 (7.1)	86	-3
160	311 (10.3)	250 (7.5)	61	257 (4.8)	54	-7
200	299 (10.2)	282 (10.9)	17	259 (6.3)	40	23
240	273 (9.8)	258 (4.9)	15	255 (7.2)	18	3
280	255 (8.9)	243 (4.7)	12	254 (7.1)	1	-11
320	239 (6.0)	228 (4.4)	11	251 (9.2)	-12	-23
360	253 (8.4)	226 (5.5)	27	252 (8.1)	1	-26
400	236 (5.1)	211 (4.7)	25	240 (4.8)	-4	-29
440	228 (6.9)	225 (5.4)	3	237 (5.4)	-9	-12
480	234 (6.4)	213 (4.7)	21	232 (4.5)	2	-19
520	227 (6.5)	223 (4.7)	4	232 (4.0)	-5	-9
560	218 (5.2)	215 (4.9)	3	240 (6.7)	-22	-25
600	229 (5.2)	230 (6.9)	-1	230 (5.7)	-1	-
640	219 (3.8)	213 (4.0)	6	232 (5.2)	-13	-19
680	223 (4.6)	231 (6.8)	-8	227 (4.0)	-4	4
720	224 (4.7)	238 (2.7)	-14	232 (6.7)	-8	6
760	228 (3.7)	257 (10.7)	-29	228 (5.1)	-	29

* In two cases where *n* would have been reduced to 19 because of premature responses, a previous response has been substituted. Bold type = significant at least at the 5 per cent. level.

A comparison between the results obtained under condition a and b.

Under condition *a* the subject is required to make—in addition to the response to the second stimulus—a motor response to a preceding stimulus which besides determining its own response also conveys information. This enables him to prepare more adequately for the second stimulus. Under condition *b* the preceding stimulus still conveys this information but the subject is not required to make a motor response. While this is not the only difference between the conditions it is thought to be the most important one and it is suggested that a comparison of the mean reaction times

FIGURE 1



Mean reaction times for last 10 responses for both hands combined.

obtained under the two conditions at each time interval gives a reasonable estimate of that element of the delay in response to *S*₂ which is caused by the need to make a motor response to *S*₁. This comparison is presented in Table I (columns 2, 3 and 4) and Figure 1 (curves *a* and *b*).

Under condition *a* (solid circles in Fig. 1) the response to *S*₂ is longer than it is under condition *b* (open circles in Fig. 1) when the interval lies between 40 and 560 milliseconds. Mean *RT*_{2*b*} is greater than *RT*_{2*a*}, however, at intervals longer than 640 milliseconds, and in the special case where both stimuli are simultaneous. The increase in the mean times under condition *a* relative to condition *b* is due to a general 5 per cent. level when *I* = 80, 120, 160, 360, 400 and 480 milliseconds. That this difference is not significant when *I* = 200, 240, 280 and 320 milliseconds, reflects the fact that the downward trend of curve *b* shows a sharp inflection with a peak at

FIGURE 2a

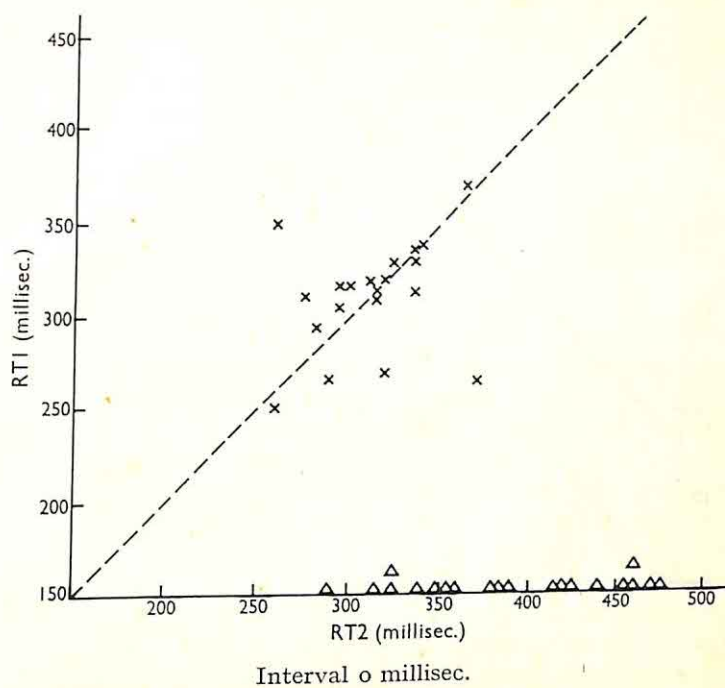


FIGURE 2b

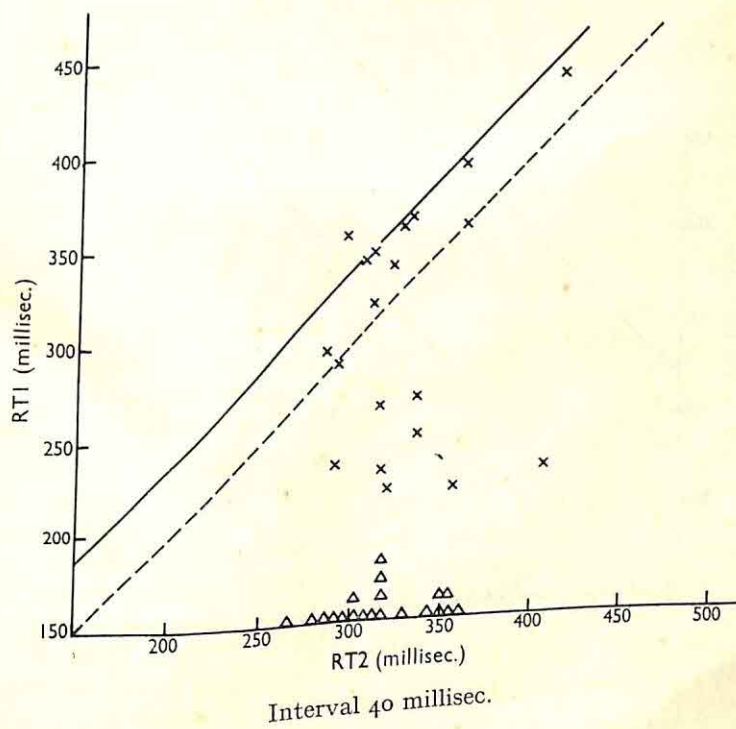
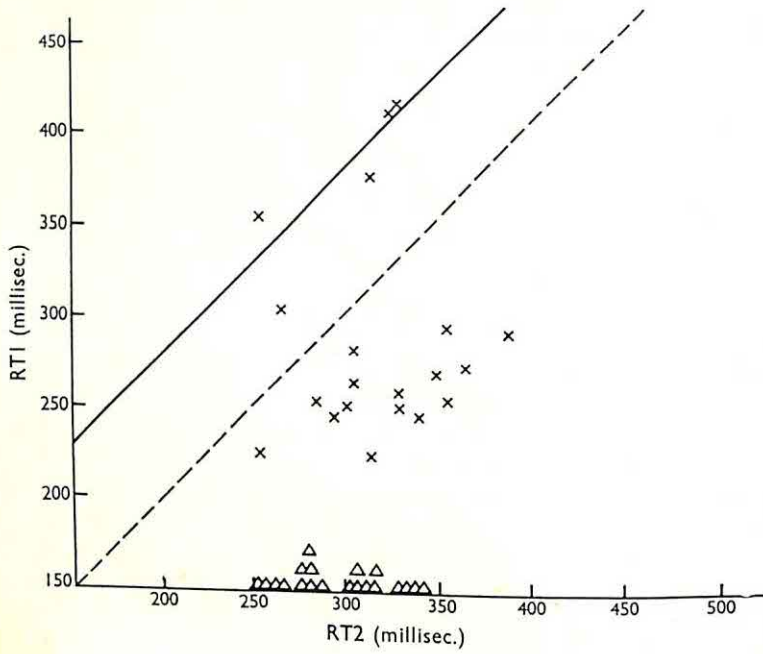
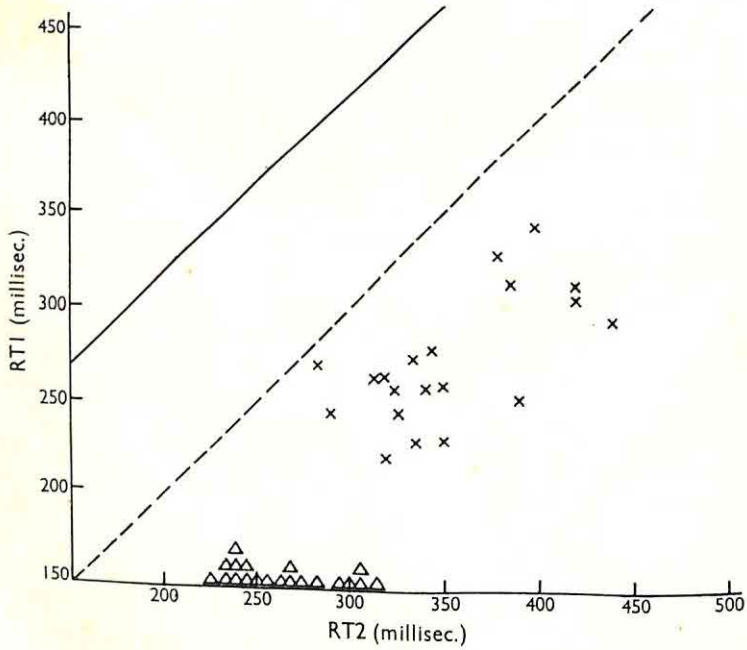


FIGURE 2c



Interval 80 millisec.

FIGURE 2d



Interval 120 millisec.

200 millisecc. The present data does not permit a detailed analysis of this inflection but it occurs in both hands and may possibly be due to an eye movement determined by the first stimulus. Leonard (1958) has reported a similar effect. In curve *a* a similar but smaller inflection occurs with a peak at 360 millisecc., i.e. about 160 millisecc. later. As already mentioned, in the special case when the two stimuli are simultaneous ($I = 0$) the mean reaction time to S_2 under condition *a* is significantly *lower* than that obtained under condition *b*. This anomalous result may be due to more than one factor. Two possible causes are:—

- (i) The difficulty of discriminating the cue stimulus (illumination of one of a cluster of five neons) from the stimulus requiring a response (illumination of the remaining four neons) when the two stimuli are presented simultaneously.
- (ii) The lower probability of having to make an overt response to a stimulus at time "0" under condition *b* (1 in 20) compared with condition *a* (11 in 20).

It is also an interesting observation that in the latter part of the range (from 680 millisecc. onwards) the mean reaction time under condition *a* is again *lower* than under condition *b*. Not only is this a consistent trend but at the longest interval ($I = 760$) the difference is significant ($p < 0.02 > 0.01$). There is no obvious explanation of this. The fact that under condition *b* there is a less intense stimulus—four rather than five neons—is not likely to be the cause since it is under this condition that the fastest times are recorded. The additional information given by S_1 is similar under both conditions and, in the case of condition *a* the response to S_1 has been completed some time previously. It is not improbable that making an overt response to S_1 produces a change in the anticipatory set for S_2 which in the present experiment persists throughout the preparatory period.

In a previous paper reporting experiments similar to condition *a* in the present experiment, it was observed that the reaction time to the second response was not—as predicted by the theory proposed by Welford—maximal at the shortest intervals (Elithorn and Lawrence, 1955). The present findings confirm this observation (Fig. 1 and Table I). Hick and Welford (1956) have suggested that this apparent exception to Welford's theory might be due to subjects grouping their responses at the shortest intervals, i.e. waiting a finite time and then responding to both stimuli together. This possibility is best tested by examining the relationships between the individuals of each pair of responses.

Characteristics of the individual responses

While RT_1 is unsatisfactory as an estimate of the undelayed reaction time for RT_2 , a knowledge of the individual RT_1 is essential to an accurate interpretation of the RT_2 with which it forms a pair.

Figures 2*a-d* show the individual results at the four shortest time intervals. At each interval the reaction times obtained under condition *a* are shown in the form of a scattergram, giving the relationship of RT_2 to RT_1 ; those obtained under condition *b* are presented in the form of a frequency distribution along the horizontal axis. In all four scattergrams the line for equal response of RT_1 and RT_2 is given (interrupted line). Above this and distant from it by the size of the interval is a solid line having the form $RT_1 = RT_2 + I$. The observations which lie along this line are those which are made at the same time and are grouped in the sense proposed by Hick and Welford (1956).

Figure 2*a* shows reaction times to stimuli presented at time interval "0," i.e. simultaneously. As neither stimulus can be said to be first in this case, the one which was designated first or second according to the system of randomization, was plotted

as such on the scattergram. In 15 out of the 20 instances the two responses may be regarded as "grouped." On the remaining occasions the differences ($RT_2 - RT_1$) were -90, -35, 25, 50 and 105 millisecon. Thus even when the stimuli are given simultaneously, grouping does not always take place.

At the next shortest interval, 40 millisecon. (Fig. 2*b*), the observed distribution of delays ($RT_2 - RT_1$) was: -60, -40, -40, -35, -35, -35, -30, -20, -10, -10, 0, 0, 50, 55, 65, 85, 90, 100, 135 and 175 millisecon. The two responses giving the first eight of the above differences were made within 25 millisecon. of simultaneity, the first response being 20 to 60 millisecon. longer than the second (i.e. RT_1 is within the range $(RT_2 + 1) \pm 20$). In the pairs giving the last eight differences in which RT_2 was longer than RT_1 , the delay was never shorter than 50 millisecon. (i.e. $RT_2 > [RT_1 + 50]$). On the remaining four occasions the two responses were equal or nearly so (i.e. $RT_2 \simeq RT_1$).

At time interval 80 millisecon. the distribution is more clearly bimodal (Fig. 2*c*), the difference values being -100, -85, -85, -60, -40, 30, 30, 30, 45, 50, 50, 60, 70, 80, 80, 90, 90, 95, 100 and 100 millisecon. At time interval 120 millisecon. (Fig. 2*d*) these values are 15, 50, 50, 55, 55, 55, 60, 65, 65, 70, 80, 80, 90, 100, 105, 105, 115, 120, 135 and 145 millisecon. As might be expected there is no grouping at this last time interval and the second responses are all longer than the first.

The delayed responses in the scattergrams show two interesting characteristics. Firstly the delays are sometimes quite small. Secondly, the lengthening of RT_2 in relation to RT_1 is not maximal at the shortest intervals. At the 80-millisecon. interval it is 50 millisecon. or less on six out of 15 occasions. At the 120-millisecon. interval it is 50 millisecon. or less on only three out of 20 occasions. At the latter interval six of the delays are longer than 100 millisecon.; at the 80-millisecon. interval none are longer than 100 millisecon. Longer delays are more common at the longer interval. This trend can be confirmed by calculating the means for the delayed responses alone. If all grouped responses are omitted then the mean times for RT_2 at time interval 40, 80 and 120 millisecon. are respectively 334.4, 325.7 and 353.3 millisecon. While the increased mean time at time 120 millisecon. is not significantly greater than the mean times at the shorter intervals, the difference is in the opposite direction to that predicted by Welford and by Davis.

Analysis of the results obtained under condition c

The results obtained under condition *c*, since they were obtained under slightly different conditions of practice are not strictly comparable with those of condition *a* and *b*. It is probable, however, that this produces only a very small change in the actual values observed and it is unlikely that it materially affects the shape of the curve. Under condition *c* the subject has to make a single two-choice response during a certain period. Under condition *b* information is given at the beginning of this period which tells the subject on which side he will have to respond, that is to say, it converts a disjunctive reaction time situation into a simple one. One would expect, therefore, that the provision of a cue would lead to a reduction of reaction times compared with the control situation in which no cue is given. A comparison of curve *b* (open circles in Fig. 1) with curve *c* (crosses) indicates that additional information given in this way can be utilized within 120 millisecon. At intervals of 80 millisecon. or less the presentation of the additional information lengthens rather than facilitates the reaction time to the *second* stimulus. Even at the shortest intervals (40 and 80 millisecon.), however, the reception and absorption of the additional information in S_1 does not slow the response to S_2 as greatly as does the need to make an overt motor response to the first stimulus.

This work was carried out while a member of the M.R.C. Neurological Unit. My thanks are due to Mrs. C. Lawrence for assistance with the experiments, to Miss R. Mingay and Miss J. Mott for help with the diagrams and to Dr. E. A. Carmichael for much encouragement and support. Mrs. M. Kerr undertook the majority of the computations and I am also very grateful to her for valuable criticism of the text.

REFERENCES

- DAVIS, R. (1956). The limits of the "Psychological Refractory Period," *Quart. J. exp. Psychol.*, **8**, 24-38.
- ELITHORN, A., and LAWRENCE, C. (1955). Central inhibition: some refractory observations. *Quart. J. exp. Psychol.*, **7**, 116-127.
- HALLIDAY, A. M., KERR, M., and ELITHORN, A. (1960). Grouping of stimuli and apparent exceptions to the Psychological Refractory Period. *Quart. J. exp. Psychol.*, **12**, 72-89.
- HICK, W. E., and WELFORD, A. T. (1956). Central inhibition: some refractory observations. Comments on the paper by Alick Elithorn and Catherine Lawrence, 1955. *Quart. J. exp. Psychol.*, **8**, 39-41.
- LEONARD, J. A. (1958). Partial advance information in a choice reaction task. *Brit. J. Psychol.*, **49**, 89-96.
- POULTON, E. C. (1950). Perceptual anticipation and reaction time. *Quart. J. exp. Psychol.*, **2**, 99-112.
- WELFORD, A. T. (1952). The "Psychological Refractory Period," and the timing of high speed performance. A review and a theory. *Brit. J. Psychol.*, **43**, 2-19.

BOOK REVIEWS

Psychology: A Study of a Science. Vol. 1. Sensory, Perceptual and Physiological Formulations. Pp. x + 710. 76s. Vol. 2. General Systematic Formulations, Learning and Special Processes. Pp. x + 706. 77s. 6d. Vol. 3. Formulations of the Person and the Social Context. Pp. x + 837. 97s. Edited by Sigmund Koch. New York, Toronto, London. McGraw-Hill Book Company. 1959.

An adequate critical notice of these three volumes could well occupy a whole issue of this *Journal*. It would be incomplete even then. There seems little point in making a list of titles and contributors and leaving the matter there. So, since a number of descriptive reviews have already appeared in journals familiar to most psychologically trained and interested readers, it seemed to me that my best plan would be to write a few general comments attempting to develop my own reaction to the project as a whole and to that part of its realization contained in these first volumes.

In 1952 the Policy and Planning Board of the American Psychological Association decided that "the time had come for a thorough and critical examination of the status and development of psychology." One part of this examination was to concern itself with problems of the training of America's psychologists and with their research productivity. These matters were dealt with in Professor Kenneth Clark's study entitled *America's Psychologists: A Survey of a Growing Profession* (1957). Another part was to deal, in seven volumes, with selected "systematic formulations of recent and current influence which psychologists have developed to account for the phenomena in which they are interested." The volumes now under review are the first three of the projected seven. They contain expositions of the points of view, and, in a rather general way, of some of the principal findings of those recent or current developments selected by the editor and his Steering Committee as representative of the present main lines of interest within psychology. A further three volumes are to deal with Empirical Substructure and Relations with other Sciences, and a final volume to be called *Psychology and the Human Agent*, will present Professor Sigmund Koch's views about "certain problems of psychological inquiry in the light of the findings of the project."

It is clear that no final verdict on either the purpose or the success of this venture can yet be passed. Much must depend upon the three volumes still to be published, which will perhaps show why, and how, increasingly closer associations are developing between psychology and other fields of scientific advance, and may have something to say about the relations of academic psychology and some of the active groups that have been established outside university control, such as operational research teams concerned with problems of military or civil significance, established by national governments or by government agencies and industrial corporations. Any reviewer of these volumes may have to eat his words later on.

All told the volumes present well over 2,000 large pages of discussion about a great variety of topics, all having something to do with psychology as it has developed during the last 30 years or so. I have read most (but not all) of this large accumulation of material, and I must confess that I am still somewhat at a loss to understand clearly just what main purpose the project was intended to serve. In spite of the general title it could not have been to show that psychology is "a" science. There are 36 contributors, each writing for the most part extremely well, though with little regard for economy of words, about his own preferred problems, approaches and methods.

Maybe the intention was to demonstrate how any sort of systematic study can be called scientific. But there is a world of difference between, say, Talcott Parsons's "Approach to Psychological Theory in Terms of the Theory of Action," together with most, if not all, of the other studies that make up Vol. 3; and Licklider's "Three Auditory Theories," or C. H. Graham's "Colour Theory" together with the bulk of the contributions to Vol. 1. Parsons begins with principles—or anyhow it looks like that—and attempts to apply them, or demonstrate their applicability in any variety of social context. Licklider, Graham and the other experimenters, write within defined, limited fields and arrive at their principles or better perhaps, at their explanations, of the related phenomena at the other end of the study, by induction from the selected contexts within their defined field. There is of course a sense in which both these approaches are scientific, but it is very hard to see that it is significantly the same sense.

Professor Sigmund Koch, the general editor has written with a devotion and enthusiasm which must command respect and admiration. But if it had not been for what he says

in his foreword to Vol. 1 and his postscript to Vol. 3, I would have thought that the real purpose of the enterprise was to present authoritative accounts of those current explorations in psychology which the editor and his selection committee regarded as typical and most important, with special, and of course, entirely legitimate, regard to American contributions. Then the books could be considered as in a line of descent from the *History of Psychology in Autobiography* sponsored by Murchison and others. Since it was suggested to the writers that they should give their essays a personal slant wherever possible—a suggestion much more faithfully observed, naturally perhaps, in Vol. 3 than in Vol. 1 and 2—this way of looking at the project would seem all the more fitting. If the main point of these publications was to inform succeeding generations about those psychological approaches of recent and current times that the A.P.A. considers now to be the most significant, the books would certainly have good and sufficient reasons for their appearance. That kind of thing ought to be done every 20 or 30 years or so, and here it has been accomplished with care and judgement. That different selections might have been made goes without saying. But this, to me at any rate, seems of very little importance. Decisions about that can be left to the reader of the future who searches the libraries for records of the present.

However, Professor Koch makes a most valiant attempt to see the contents of these books "unified by a relatively homogeneous set of attitudes towards the ends and instrumentalities of scientific work." His views are to be fully expounded in the seventh volume, but something, perhaps, ought to be said about them now. He finds the unity he wants by putting all the contributions into a framework of generalized titles. Up to about 1925 he sees psychology as in an "era of schools." Then a transition occurred to an "Age of Theory" and there we may have been living ever since, though with many attempts to break out in varied directions. But this, like a great deal more in the "Epilogue," is not quite clear, for he also says that now it is anybody's guess "whether we are in the Age of Theory or not." However the Age of Theory has its "premises" and these are said to be:

- (1) The Intervening Variable Paradigm for Theory Construction.
- (2) Problems concerning the Generalization Range of Psychological Laws.
- (3) The Observation Base of Psychological Science; Its Relation to the Legitimate End-terms of Systematic Analysis.
- (4) Mathematization of Systematic Relationships.
- (5) Formulization and Psychology.

Under these highly abstract headings, and with abundant reference to the preceding essays, Professor Koch seeks to show what are the general trends, not of these publications alone, but of psychology, during the last 30 years. In the end he doesn't seem quite sure. The bother is that the experimentalists, especially those concerned with sensory problems, refuse to fit neatly into any such scheme. Unfortunately, I think, Professor Koch sees this almost entirely in a negative way: "the results of Study 1 set up a vast attrition against virtually all elements of the Age of Theory code."

Naturally the editor was bound to deal principally with psychological developments as they are presented in these volumes. That is to say his remarks are predominantly determined by American contributions. All the time work has been going on elsewhere, and if a scheme is put forward which claims to deal with psychology as a whole, some of this surely deserves serious consideration. Maybe not only the simplest, but also the most satisfactory way of looking at what has happened is to agree that the same sorts of influences have slowly become operative in psychology as in the other natural sciences: a growing readiness to accept experimental control and criteria and a recognition that it is legitimate to mark out a particular field of study and to work within that without being over much hunted by the demand that methods and results—especially results—should be equally applicable to all other fields of psychological study. Obviously these developments have been most marked in the fields of study of the special senses and perception, but they are steadily becoming more effective in the investigation of all high level individual behaviour. They have been least successful in social psychology. More and more attempts are being made all the time, however, to use the same methods and criteria in these directions also, but how far these attempts can go, or what they may ultimately achieve, nobody can yet say.

I think this would be a simpler way of looking at contemporary developments in psychology than Professor Koch's and one requiring a much less twisty and difficult justification. This is not, however, to under-value what he has done. Much of his

discussion is stimulating and idea-provoking, though I think he could have used fewer and often shorter, words with advantage.

Finally let me repeat that the level of scholarship in the many individual contributions to these volumes is consistently high throughout.

F. C. BARTLETT.

Current Problems in Animal Behaviour. Edited by W. H. Thorpe and O. L. Zangwill. London. Cambridge University Press. 1961. Pp. xiv + 424. 45s.

In 1953, it appears, a small group was set up in Cambridge to bring together ethologists and psychologists for discussion of their common problems, a cold war being changed to a hot one, one might suppose. This book, however, is not a record of the conflict, and not even a peace treaty, a term which inevitably implies some opposition of aims. It records instead the fact that the aims of the contestants are discovered to be the same, and their methods complementary. In 1953 psychologist and ethologist *were* at loggerheads, but not in 1960, and there is some reason to think (though the Editors do not claim this credit for the enterprise) that the Cambridge group had something to do with the change. For anyone who wishes to be informed on such developments (or any psychologist who thinks that ethology still makes an antithesis between learning and instinct), and for all who are interested in the comparative problem of behaviour, some of the chapters in this book are necessary reading. The chapters are a mixed bag, rather lacking in over-all unity, and not all equally interesting—at least, not to any one reader. The content varies from (a) the kind of research which describes and catalogues the way in which a couple of hundred species of birds flick their tails to (b) an attempt to resolve the continuity-noncontinuity problem in terms of information theory. If the Editor will permit the figure of speech, it seems here and there that an author has got the bit in his teeth and ridden off on his own private hobbyhorse. But if one chapter is not to your taste, read on: another may be very exciting.

The original small group was enlarged, with variable membership, over a period of years. As it became apparent that useful results were emerging from the arguments, the members were asked to submit papers for the group to criticize, and then to prepare for publication any remnants of their ideas that might still be left. Thus we have an unusual symposium: no hit-and-run reports of research, and no undigested spur-of-the-moment discussion presented verbatim. This makes for clarity and avoids the trivial; but it may make for dullness too, the more thin-skinned contestants perhaps avoiding dangerous topics or leaving out any points that might lead to argument, and parts of this book are undeniably dull.

The contributors and their titles are as follows: D. M. Vowles, Neural mechanisms in insect behaviour; L. Weiskrantz, Encephalization and the scotoma; O. L. Zangwill, Lashley's concept of cerebral mass action; A. D. Blest, The concept of ritualization; J. H. Crook, The basis of flock organization in birds; P. Marler, The filtering of external stimuli during instinctive behaviour; R. A. Hinde, The establishment of the parent-offspring relation . . . ; W. H. Thorpe, Sensitive periods . . . imprinting [and] cyclic behaviour; M. A. Vince, Developmental changes in learning capacity; D. E. Broadbent, Human perception and animal learning; A. J. Watson, The place of reinforcement in the explanation of behaviour; R. L. Gregory, The brain as an engineering problem; H. B. Barlow, The coding of sensory messages; and V. Cane, Some ways of describing behaviour.

The chief value of the book, for this reviewer, is in presenting ethology to the psychological community. We are shown a picture of ethology on the move, sophisticated in method and theoretical conception, and concerned to deal with the mechanisms of learning as well as with behaviour that is predictable and diagnostic of a species. The book should have a place on the shelf beside Tinbergen's *The study of instinct*: not a replacement, but an important addendum. From this point of view the papers by Hinde, Marler, Thorpe, Vince and Vowles are of special interest, together with the introductory statements (to the whole book and to its several sections) by Zangwill and Thorpe.

If the book constitutes an exchange of ambassadors, however, the picture of psychology that is given to the ethologist is less representative, more fragmentary. Weiskrantz reviews the evidence for an encephalization of visual function and finds that no great change has occurred during mammalian evolution. Zangwill critically considers the theory of mass action and equipotentiality, with suspended judgement, finding things to support both Lashley and his critics. In the present context, these two papers appear as isolated *tours de force*; they have a certain continuity with the preceding paper by Vowles (brain operations in the ant!), but lack any real follow-up in later chapters. In themselves, however, these two critiques are very good. Broadbent is less convincing

in his argument that reactive inhibition is all perceptual, and in his attempt to show that the continuity-noncontinuity problem in learning theory can be made to vanish by the conception of a filter in input-output transmission. A valid idea is asked to carry too heavy an explanatory load, and the whole problem of equivalence is disregarded or dealt with at a verbal level: see for example, the argument (p. 258) that to a chaffinch an owl and a stoat are perceptually the same, or similar, because both are dangerous: "exposure to one predator-stimulus lowers the probability of any response to another predator-stimulus" as reactive inhibition sets in. Watson reviews the theory of reinforcement in terms of drive reduction, and finds against it. The review is very capable but, really, we already knew that the theory was bad, and what we need in this area is some risk-taking in the form of positive explanatory ideas. Condemnation of theory is easy. Gregory seems to be giving us a number of reasons why we cannot hope to learn how the brain functions until we know more about the brain, and comes close to saying that we can learn nothing by stimulation and extirpation methods—ergo, we have learned nothing in the past. On localization of function, where he uses the example of speech and aphasia, his discussion seems to suffer from considering only the production of speech sounds: from this point of view speech is no more "localized" in a cortical area than in the vocal cords; either is essential. But aphasia involves also reading, writing, and speech comprehension. "Localization of function" taken literally is a poor conception, no doubt, but so far we lack a better with which to comprehend certain definite facts.

The reader may be grateful to Gregory and Broadbent, however, that they have not gone pussyfooting about their assignments. Such discussions will arouse contention, as they were clearly meant to do, and the clear and incisive statements will help the reader who dissents to clarify his own ideas.

In sum, this book presents some essays in psychology that are wise and informative, and some that are stimulating and provocative (or provoking). They contain much of value. To the present reviewer, however, a greater value is in bringing him more nearly up to date on attitudes and opinions in the ethological realm. The book is recommended to all who are interested in the problems of animal behaviour, whether "comparative psychologists" or not.

D. O. HEBB.

Perspectives in Personality Research. Edited by H. David and J. C. Brengelmann. London. Crosby Lockwood. 1960. Pp. xii + 370. 50s.

This is a collection of papers on contemporary researches in the field of personality, most of them given at the 15th International Congress of Psychology at Brussels in 1957. The editors deserve our gratitude for rounding up the authors and translating foreign language papers, though they might have provided a more adequate index. Not the least valuable parts are commentaries on the whole volume, or on groups of papers, written by Henry Murray, S. S. Tompkins, R. B. MacLeod and R. Meili. It gives a good sampling of current interests, though no more so than, say, an issue of the *Journal of Abnormal and Social Psychology* and one or two other journals; indeed it is less satisfactory in that papers prepared for short lectures seldom provide full details of any investigation reported. An alleged advantage is that the volume represents an international coverage; in fact a good three-quarters of the space is taken up by Americans or those working in the American tradition. Nevertheless From's (Danish), Brengelmann's (English) and Meili's (Swiss) contributions certainly strike some fresh notes. (A paper by Luria on the role of the secondary signalling system in the development of voluntary action, though obviously interesting, seems to have strayed here by accident.)

Presumably readers of this *Journal* are concerned to know whether the field is nowadays intellectually respectable, whether it follows the canons of sound scientific enquiry. I think the answer is yes, though the book provides little direct evidence. Only one contributor, D. R. Miller, describes a well-controlled experiment, namely a study of the psychoanalytic concept of defence, where the tendency to deny or distort in (projective) conflict situations was shown to be linked with sex, age, social class, type of home discipline, and fundamentalist religious training. Somewhat similarly G. Blum attempts to build up a cybernetic model of psychoanalytic behaviour theory and to test it out by experiments with psychosexual stimuli, namely the Blacky Pictures.

What then does the field of personality encompass? Definitions would doubtless differ, but I would suggest as a rough answer: systematic studies of emotional factors that produce differences in behaviour from one individual to another. This would rule out physical differences except in so far as somato-types or brain injury, for example, influenced basic temperament; ability differences would likewise be excluded. Attempts

to put psychoanalytic and other motivational theories on a scientific footing could be included, though therapy would be only an applied derivative. Attitude studies would certainly be relevant, even if they equally concern the social psychologist. Diagnosis and assessment, together with factorial analyses, have been a major area for over 40 years; but the fact that so little progress has been made suggests that these are premature until sounder theoretical formulations are available. More fruitful, perhaps, are investigations of the influence of personality on perceptual and motor differences (the "new look" in perception, and expressive movements).

The following contributions, then, are legitimate, though not very inspiring examples. I. E. Alexander and A. M. Adlerstein's investigation of death correlations by free word association and questionnaire techniques, H. P. David's and W. F. Floyd's survey in the *Psychological Bulletin* manner of brief projective devices (180 references), E. S. Shneidman and N. L. Farberow's investigation of socio-economic differences in types of suicide, and Brengelmann's experiments on individual differences in the development from a vague to a clear percept, which he shows to depend markedly on the precise nature of the experimental set-up. Much of the work done on cross-sectional or longitudinal studies of child development and ageing likewise contributes to the field of personality, though the extreme complexity of the determinants of individual differences makes the results so far appear rather meagre. In this volume there is a symposium on longitudinal studies by B. Inhelder, L. B. Murphy, A. G. Skard and others, but the abstracts given are too brief to be very informative.

The most stimulating section is a series of papers on perception of personality. MacLeod argues the need for an adequate theory of perception and believes that personal attributes and purposes are perceived in very much the same sense as are physical objects. (Michotte's work on perception of causality in moving geometrical figures might be taken as supporting evidence.) An investigation by R. Tagiuri suggests that a genuine psychophysics of personality perception might be developed; his subjects were shown a series of lines—straight, curved, wiggling—which they were to imagine as paths followed by a person walking from A to B, and asked to describe the attributes of the person who took each path. Considerable consistency of judgement was obtained, indicating that specific features of an expressive movement can be linked with specific person perceptions. MacLeod suggests, however, that, in our present state of knowledge, the more descriptive approach of continental writers may be more fruitful than the quantitative approach of American ones. F. From presented short films of a person performing various actions and showed that the perception of purpose often determined the perception of movements. He concludes that it is fallacious to assume that we can observe or study behaviour as such; in daily life it is always subordinate to our interpretation of the person who behaves. To the experimental psychologist, no doubt, this approach seems more anecdotal than scientific. Nevertheless, it readily leads to a variety of testable hypotheses, which seem a good deal more likely to advance our knowledge of personality than does most of the work with questionnaires, projective and objective tests. In another contribution, R. Taft shows that quite a lot has been established regarding the characteristics of judges of personality, their use of empathy, of cultural stereotypes and so forth.

E. P. Hollander carries the matter further with a discussion of Crutchfield's well-known experiments on the conformity tendency. These results have been interpreted too readily as indicating a personal attribute or trait, neglecting the situational determinants. More productive work on personality would concern itself with interactions between situationally-determined behaviours and inner dispositions. Finally Meili contributes a wise discussion of the pitfalls which have bedevilled so much work with personality tests; and, in a characteristically urbane introduction, Henry Murray essays an outline of some future comprehensive synthesis of all the varied theories and investigations of personality.

One concludes the book, then, with a feeling that the field is still a messy one—far too complex for any rapid progress with the available methods, and yet that much greater insight is being shown into its difficulties and that much solid work can be, and is being, done in many parts of the world.

P. E. VERNON.

The Psychology of Affiliation. Experimental Studies of the Sources of Gregariousness. By Stanley Schachter. London. Oxford University Press (for Stanford University Press, Stanford, California). 1959. Pp. 141. 30s.

This book has the laudable aim of trying to state the conditions in which individuals seek other people's company or prefer to be by themselves. The investigation was carried

out by means of experiments in which anxiety was reputedly manipulated, and by examining data from such differing social situations as fighter aircraft, a nursery school and psychotherapy sessions.

The experimental studies demonstrated that the level of "anxiety" was correlated with the desire to be with others rather than to stay on one's own; anxiety is in quotation marks because the author persistently side-steps the perplexing question of defining anxiety. The subjects were, in fact, forced to *choose* how they wished to spend their time in the anxiety-provoking situation, and not observed actually reacting to an unstructured social situation. From the experiments the author suggests that when other people are sought out in these conditions, it is to enable the subject to compare his personal feelings with those of others. The data from the field studies mentioned above are brought in to support probably the most striking result of the whole investigation—that birth-order in the family is not randomly distributed in these situations, but skewed towards first and only children either seeking social situations or performing less well in lone situations, than late-born children.

This reviewer feels that within the highly constrained experimental situations—using American students—the author has substantiated his thesis. However the field evidence is less easy to accept because of the assumptions about the social meaning of the types of behaviour which are described.

D. F. HOOPER.

Medical Electronics: Proceedings of the Second International Conference, Paris, 1959.
Edited by C. N. Smyth. London. Iliffe & Sons. 1960. Pp. 614. 145s.

This book comprises the papers given at the first full-scale international conference on medical electronics, which was held at the new UNESCO building in Paris, in 1959, when about 160 papers were presented. The whole undertaking owes a great deal to V. K. Zworykin, who in recent years has turned his interest and his inventive genius to medical electronics. The book represents an enormous amount of work by particularly C. N. Smyth, A. Remond and B. Shackel.

Topics covered include electrophysiological techniques, electroencephalography and eye movement recording, as well as new instrumental techniques, such as the use of ultra-sonics in place of X-rays to produce images, an ultra-violet flying-spot microscope, and the use of computers to aid medical diagnosis.

The conference reflects the present lack of active interest by doctors in electronic techniques: communication and ideas are almost all one way. If medicine does really take to computers and precise measuring techniques, it seems possible that the relative status and responsibility of doctor and clinical psychologist will change, and this book gives an insight into what medicine could become in the near future.

R. L. GREGORY.

Medical Physics. Vol. 3. Edited by Otto Glasser. Chicago. The Year Book Publishers Inc. Pp. 754. \$25.00.

This is the third volume of what is essentially an encyclopedia of a rapidly growing science, or set of sciences. This current volume brings up to date earlier contributions, as well as introducing new topics. It includes an index of the previous volumes, but it can be regarded as a complete work in itself.

The range of topics is vast—e.g. artificial organs, space hazards, psychophysics—and the standard of clarity and precision is exceptional. This book is of real use for accounts and bibliographies of specific recent work, such as Lawrence Stark on the pupil reflex and Christine Kris on electro-oculography. The contributions on more general topics suffer from inevitable over-compression.

The cause of medical physics is well served by this beautifully produced volume. It is distributed in this country by Interscience Publishers Ltd., at 188s.

R. L. GREGORY.

Das Gestaltprinzip im Leben des Menschen und der Tiere. By Karl Bühler. Enzyklopädie der Psychologie in Einzeldarstellungen. Vol. 5. Bern. Hans Huber. 1960. Pp. 106. DM 22.

In this attractive (if admittedly slight) little volume, the venerable Dr. Karl Bühler looks afresh at the Gestalt problem, his scholarly eye ranging over the whole spectrum of psychological theory from von Ehrenfels to von Neumann. Gestalt perception is

viewed by Dr. Bühler as a specific accomplishment of the organism and as in some sense the prerogative of biological (as opposed to cybernetic) systems. It is doubtful, however, whether this view will commend itself to many psychologists today. At the same time, Dr. Bühler has some interesting things to say about ethology, patterns of bodily growth and even the reticular formation! Clearly his contact with modern work has been in no wise dimmed by age. In translation, this little book would make an admirable Pelican.

O. L. ZANGWILL.

Handbook of Research Methods in Child Development. Edited by Paul Henry Mussen. London. John Wiley & Sons, Inc. 1960. Pp. x + 1061. 122s.

This vast and rather dispiriting compilation is divided into five sections devoted respectively to general research methodology, biological growth and development, cognitive processes, personality development and social behaviour and environment. With the exception of two of Piaget's collaborators all the authors are American and little or no account is taken of genetic psychology in the Soviet Union. Nor is there much reference to the special problems that arise in the study of handicapped children. At the same time, this volume does have value as a guide to those who are beginning research in the developmental field and there is a good case for libraries buying it.

O. L. ZANGWILL.

The Sounds of Language. By L. F. Brosnahan. Cambridge. W. Heffer & Sons Ltd. 1961. Pp. vi + 250. 25s.

When Professor C. D. Darlington, in 1947, put forward a specific theory concerning one of the genetic components of language, his work was criticized on various grounds, especially by Professor L. Hogben, but many people still believe that there is substance in the idea. One of the difficulties in evaluating such a theory was that few people are well enough informed about both linguistics and genetics to be able to have firm opinions as to the biological meaning of distributions of languages and their sounds.

In the present book, Dr. Brosnahan has gone far to remove one source of uncertainty. The chapters on Phonetic Change and upon Geographical Distribution of Speech Sounds, in *The Sounds of Language*, are full of information presented in scholarly fashion. Moreover, the discussion of anatomical differences in the vocal apparatus, which are likely to be genetically determined and which are not homogeneously distributed in the human race, is clear and sensible. The author also points out correctly that hereditary variations in the structure of the hearing apparatus must be significant.

Troubles arise, however, when the attempt is made to find supporting evidence for Darlington's theory that the blood group gene O is directly responsible for facilitating the acquisition, if not the persistent retention of the "th" sound, the vocal fricative. The crux of the argument is provided by the two maps, on pp. 56 and 57, where the past and present distribution of this linguistic sound in Europe and the O-gene distribution are compared with one another. There is undoubtedly a correlation between the two. Unfortunately Dr. Brosnahan's genetical knowledge is not sufficient for him to perceive that this association cannot prove that the gene O is the hereditary factor responsible for the linguistic character. The kind of information required to prove this point would be obtained if it were found that, within the same community or family, those members who possessed gene O were better able to pronounce "th" or more attracted to the sound than were members who lacked it, such as people, for example, with AB blood group. Arguments derived from distribution associations might be used erroneously to support all manner of hypotheses about the dependence of stature, cephalic index, eye colour, religion, politics or dress upon blood groups or similar traits. Fortunately the association between one sound and one blood group gene is the only one so far advanced in this way as serious evidence for genetical causation.

In spite of this misconception about genetical inferences, the author has written a book which is interesting, informative and stimulating.

L. S. PENROSE.



INDEX OF AUTHORS

[illegible]

SPONG, P., with Sampson, H.	- - - - -	173
STANDFAST, S., with Chase, R. A., Harvey, S., Rapin, I., and Sutton, S.	- - - - -	141
STORY, A. W.	- - - - -	204
SUTHERLAND, N. S.	- - - - -	- 117 and 222
SUTTON, S., with Chase, R. A., Harvey, S., Standfast, S., and Rapin, I.	- - - - -	141
SUTTON, S., with Chase, R. A., Rapin, I., Gilden, L., and Guilfoyle, G.	- - - - -	153
TERWILLIGER, R. F.	- - - - -	209
TREISMAN, A., with Davis, R., and Moray, N.	- - - - -	78
TREISMAN, M., with Howarth, C. I.	- - - - -	12
WEISKRANTZ, L., with Gross, C. G.	- - - - -	34
WILSON, J.	- - - - -	218
WYCKOFF, L. B., with Zeigler, H. P.	- - - - -	129
ZEIGLER, H. P., with Wyckoff, L. B.	- - - - -	129



